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THE QUANTITATIVE STUDY OF ORGANIC REACTIONS[1]

BEFORE this body of chemists it is not necessary to call attention to the importance of many of the great researches in the field of so-called pure organic chemistry. The investigations of Liebig and Woehler and Fischer on uric acid derivatives, of Friedel and Kraft and others on the use of aluminium chloride, ferric chloride and zinc chloride in effecting many condensations; of Sandmeyer and Gatterman, Hantzsch, and Bamberger on the formation of diazo compounds and their derivatives; of Baeyer, Greene, Nietzki, Fischer and others on the formation of dyes; of Willstaetter, Pictet, Koenigs, and Pschorr on the alkaloids; of Wallach, Tiemann, Semmler and Harries on the terpenes; of Fischer, Kiliani, Tollens, Bruyn, Wohl and Ruff on the sugars, and of Fischer, Abderhalden, Neuberg, Curtius, Kossel, Osborne and Chittenden on the proteid compounds—all of these great researches speak for themselves in their importance to pure science, technology, medicine and the biological branches.

However much the physical chemist may turn up his nose, or hold it, in the organic laboratory and call us *pot-boilers* and *stink-producers* and *mere compound-makers;* however much the clean-working analytical or inorganic chemist may rail at us because we do not, as a rule, collect and weigh our organic precipitates accurately to within 0.10 per cent., yet these same deluded col-

[1] Address of the chairman of the Division of Organic Chemistry in Section C of the American Association for the Advancement of Science, Baltimore, 1908.

leagues seize eagerly upon our organic solvents for use in their inaccurate methods for the determination of molecular weights, and upon our organic indicators to aid them in poor analyses. They clamor for, and would die without, our medicines, such as whiskey (*imitation* or any kind), atropine, quinine and ether, our cane sugar and glucose and our artificial breakfast foods; and their dainty daughters could not so entrancingly lure sweethearts without our beautiful dyes in their silks and cheeks, our delightful synthetic violet perfume in their handkerchiefs and our artificial pineapple, strawberry and vanilla in their dangerous embryonic pastries and ices. This same compound-making, this so-called *old style* organic chemistry, with its reactions and methods, is not only of the very greatest importance to science and economy, but is becoming more so every day.

But we have another side to organic chemistry, which appeals to the physical and inorganic chemists, and so much so, indeed, that they try to appropriate it in their text-books as part of their own domain: namely, the quantitative study of organic reactions. In pointing out the problems that busy our organic chemists, perhaps I can not do better than to bring to your attention some of the researches that have been, or are being, completed and still other problems whose solution is of the greatest importance to the chemical world.

The question of fundamental importance in any reaction, and the one that must be answered first, is: *What are the constituents that are really uniting or reacting to give the end products?* When this is known *we must then determine quantitatively the natural constants which govern the concentrations of the reacting constituents at any time and their velocity of transformation and final equilibrium.* These constants are just as characteristic as are the melting points and molecular weights for each set of substances under definite conditions, that is, in definite concentrations, in definite solvents, at definite temperatures and exposed to definite wavelengths of light of given intensity, etc.

The question as to what are the really active constituents of a given reacting mixture, whether it be one of organic or inorganic substances, is not always easily answered. Do the molecules which we put together unite or do their simple or complex ions enter into combination? Some of our prominent physical chemists tell us "that most chemical reactions, if not all, are reactions between ions; molecules, as such, do not enter into the reaction at all." On the other hand, other prominent physical chemists go perhaps too far in the other direction: they elect their colleagues with brotherly love to life membership and exalted rank in their own Ananias Club and declare that "whoever claims that the instantaneous chemical changes in aqueous, or other, conducting solutions take place because of the fact that these solutions are electrolytes (or in current phraseology, because they contain ions), must assume the burden of proving his proposition," a statement to which we must all agree. Both of these factions of physical chemists, I believe, fail to weigh judicially all of the experimental evidence at hand. The champions of the union of ions as the cause of chemical reactions argued from such facts as the failure of dry hydrochloric acid and dry ammonia to form ammonium chloride. We can challenge them to prove beyond question (1) that the molecules do not react in the presence of moisture, (2) that the gases were perfectly dry, (3) that the gases did not actually slowly unite. They could answer that no *apparent* union took place in a great length of time: but we could

recall to them that neither do dry hydrogen and oxygen *apparently* unite at ordinary temperatures, even in the presence of platinum. Yet Bodenstein has proved that they *do* unite appreciably at temperatures above 500° C. in the presence of platinum, and he has calculated that if the observed temperature coefficients hold it would require the volumes of hydrogen and oxygen with which he worked *several hundred years to form a small fraction of a milligram of water at ordinary temperatures.* We sincerely hope that our ardent admirers of ions will start these experiments for their posterity to finish as a monument to them. On the other hand, the defender of the divine right of molecules to enter into instantaneous chemical reactions ignored the small minority of the ions present with the large number of molecules in such mixtures as hydrochloric acid and copper oleate in benzene solution. It does not follow that because such solutions hardly conduct the electric current there may not be present, in practically instantaneous equilibrium with the molecules, *minimal amounts of ions which react with each other with enormously high velocities.*

This point is perhaps best illustrated by the beautiful work of Whitney, Melcher, Kuester, Bodlaender and others, on solutions of the complex cyanides and ammonium compounds of copper, cadmium and silver, in which only minimal traces of the silver, copper and cadmium ions exist. A normal solution of $KAg(CN)_2$ contains so few silver ions that the dissociation constant,

$$K=\frac{Ag(CN)_2}{Ag\times(CN)^2},$$

has the enormously high value 10^{23}, which expresses in liters the volume of water which flows over the Niagara Falls in *500,000,000 years!* Yet the silver can be electrolyzed out of this same solution in a very short time. The dissociation constant of the complex cadmium cyanide ion,

$$K=\frac{Cd(CN)_4}{Cd\times(CN)^4},$$

is 10^{17} and that of hydrogen sulphide,

$$\frac{H_2S}{H\times SH}=K,$$

is 1.75×10^7, and yet in a mixture of solutions of these two substances the cadmium sulphide is precipitated almost instantaneously. Indeed it seems possible that ions with the mass which we are accustomed to ascribe to them must be so far apart in such solutions that they can not possibly migrate rapidly enough to allow all of them to come together in such a short time, and it may be that further investigation will show that these complex cyanides and ammonium compounds react in some cases through their complex ions or molecules. Kahlenberg has done great service to physical chemistry by insisting that some reactions may be those of molecules. It is quite evident then that in such inorganic reactions, which usually take place with very high velocities, it is sometimes very difficult to decide whether the constituents directly concerned are molecules or ions.

But organic chemistry offers us a rich field in which all kinds of reactions can be found. Most of these take place comparatively slowly and can be studied easily, and the values of the various constants can be determined accurately. In some reactions we have already shown that both ions and molecules undergo transformation. Of course many organic reactions do not lend themselves to quantitative work, on account of complex or disturbing side reactions which can not be followed accurately, or on account of the difficulty of finding a good analytical method for determining the concentration of each constituent at any moment. But it is certain that we could not have had the best development of the law

of mass-action, of reactions of the first and second order, of the development of temperature coefficients, of Thomson's study of energy changes in combustion, etc., without the quantitative study of such organic reactions as the inversion of cane sugar, hydrolysis of methyl acetate, etc.

It will, perhaps, not be uninteresting to discuss some of the problems that are at present occupying the attention of organic chemists. The phenomena exhibited by the so-called *tautomeric compounds* are of great importance in a large number of substances, such as quinazolines, cyanides, sulphites, cyanates, amides, acetoacetic ester, pyrazolones, uric-acid derivatives, pyrimidines, all kinds of dyestuffs, such as the rosanilines, phenolphthaleins, fluoresceins, etc. In 1899 we had several theories of the action of tautomeric compounds with other substances like alkyl halides and acid chlorides. Those of Nef, Michael, Wheeler, Comstock, Knorr and Wislicenus deserve special mention.

Comstock assumed that the potassium salt of an amide, for example, and an alkyl halide yield only one ester, the N-ester, because the potassium salt is the N-K salt (see the graphic scheme below!) and that the silver salt is an O-Ag salt and yields the O-ester. Wheeler assumed that all salts of amides are O-salts and that these salts yield the O-ester primarily, which then rearranges *per se*, or is transformed catalytically by the alkyl halides into the N-ester. Nef and Michael assumed that all salts are O-salts and that the O-ester is formed by a direct replacement of the metal, while the N-ester is formed by the primary addition of the alkyl halide to the N, or to the double bond between the N and the C. Wislicenus and Knorr believed that isomeric esters are formed by the action of alkyl halides on such so-called tautomeric salts because there are present in the molecule or ion of the salt oscillating free bonds or valences which can bind the entering alkyl group at different positions. This point of view has been used brilliantly in recent years by Lapworth.

All of these theories had much to recommend them, and each one harmonized with most of the qualitative facts known at that time. Another point of view, however, seemed to the speaker more plausible, namely, that in some cases isomeric stable esters are formed by the action of the alkylating agent on two or more tautomeric salts in equilibrium, in other cases the esters undergo rearrangement, and in other cases the anions (perhaps the molecules, too) add the alkyl halide at two different places.

Now obviously a large amount of quantitative work was an absolute necessity if we were to decide which of the above several theories is right. It was seen, too, that the chemists who undertook to solve the problem would have to work for *years* on the quantitative side before they could tell their colleagues the mechanism of these tautomeric reactions.

It may be stated that quantitative work in this particular field is very expensive, difficult and tedious, many operations being necessary to give us one set of data. If we had not been assisted so generously by the Carnegie Institution of Washington we should have made very, very slow progress. The investigations of the speaker and of Messrs. Laist, Willcox and McRae, of the University of Utah, and of Drs. Brunel, Johnson, Rogers, Shadinger and Nirdlinger, of this laboratory, during the past nine years, on the urazoles has at last brought us to the conclusion that our theory of tautomeric salts is at present the best working hypothesis for the explanation and further study of such reactions. This theory has been stated[2] as follows:

[2] For references see *Berichte der deutschen chemischen Gesellschaft*, **41**, 3199.

The weakly acid tautomeric amides, acetoacetic ester, etc., react, as a rule, only very slowly with alkyl halides, acid chlorides, etc., at ordinary temperatures, because these weak acids furnish few *anions* of the tautomeric forms to react with the alkyl halides. The addition of bases such as pyridine, alkalies or other metallic hydroxides, or sodium ethylate, causes an *increase* in the reaction velocity, or a *catalysis,* because the base forms, to a greater or lesser extent depending upon the strength of the acid or base, tautomeric salts of the amide, acetoacetic ester, etc. These salts, because they are more highly ionized than the acid, furnish the solution with a greater concentration of the *anions* of the two or more tautomeric forms, and hence the reaction velocity is increased. This acceleration is exactly analogous to the catalysis of amides, oximes, esters and cane sugar by acids in water, in which, according to the theory developed by Kastle, Bredig, Lapworth, Acree, Goldschmidt, Stieglitz and Euler, the *cations* of these substances are hydrolyzed. The free bases (amides, oximes, etc.) are so weak that they furnish only a few *cations* ($CH_3C\dot{O}NH_3 + \bar{O}H$, etc.). But when acids are added these weak bases unite with them and form salts to a greater or less extent. These salts are highly ionized and hence furnish the solution with a much greater concentration of *cations,* and the reaction is accelerated merely on account of this greater concentration of *cations.*

This theory has enabled us to predict mathematically and prove experimentally the following propositions:

1. The old equations used by Ostwald and modified by Hantzsch and Stieglitz, to express the dissociation constants of such tautomeric acids and salts, especially in the phenolphthalein series, were wrong because these workers embodied in them only the ionization of one of the tautomeric forms; our point of view led us to equations for these dissociation constants which are acknowledged by the other workers to be correct and which have been tested experimentally by our own work and that of Wegscheider.

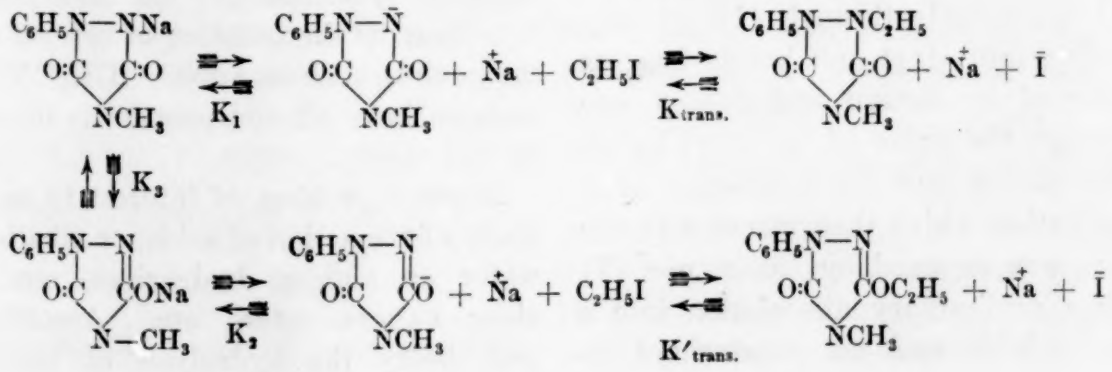
2. Our point of view on tautomeric compounds led us to a correct interpretation of the phenomena of abnormal hydrolysis which we have tested experimentally by reaction velocity methods.

3. The alkyl halide reacts with the anions and not appreciably with the molecular form of the salt.

4. The equilibrium is between the molecular forms of the salts and not between the anions as assumed by Knorr, Lapworth, Nef, Wislicenus and others. In this case, then, we have a reaction of molecules, and not of anions.

5. Our theory enabled us to predict and verify quantitatively, experimentally, the following important facts in the formation of isomeric esters from various salts of urazoles and different alkyl halides:

(*a*) Different salts and the same alkyl



halide give different ratios of N-ester to O-ester and react with different velocities because the various salts have different values for K_1, K_2 and K_3.

(*b*) The value of K_3 is not changed appreciably by a change in temperature and therefore the ratio of N-ester to O-ester is not changed. But the values of K_1 and K_2, on the one hand, and of $K_{trans.}$ and $K'_{trans.}$, on the other hand, are altered practically alike and hence the velocities of formation of both N-ester and O-ester have about the same temperature coefficients as other similar reactions.

(*c*) A change in solvent may cause a change in K_1, K_2 and K_3, and hence cause a change in the ratio of N-ester to O-ester.

(*d*) The same salt and different alkyl halides yield different ratios of esters, because $K_{trans.}$ and $K'_{trans.}$ differ for the different alkyl halides (see *g*).

(*e*) A change in the concentration of the salt and the alkyl halide does not appreciably change the ratio of the N-ester to the O-ester because there is no appreciable change in the ratio of the ions from the two salts.

(*f*) The addition of another salt with the same cation produces no appreciable change in the ratio of the N-ester to the O-ester obtained from a given salt and an alkyl halide, because the degrees of ionization of both tautomeric salts are suppressed practically alike.

(*g*) The alkyl halide reacts with the anion; but the alkyl halide does not seem to react appreciably through (1) primary dissociation into alkyl and halide ions, as was assumed by Bruyn and Steger, nor (2) through the union of the alkyl halide with the cation and the formation of a complex cation which then reacts with the anion, as was assumed by Euler, nor (3) through a preliminary dissociation into a halogen hydride and an unsaturated alkylene or alkylidene residue, as was assumed by Nef. But mathematical reasoning alone led us to the conclusion that the alkyl halide may unite to a small extent with the urazole anion and form a complex anion which then yields the ester and the halide ion. A fundamental part of this problem is: (1) to determine experimentally the relation of the amount of the free energy of the different isomeric alkyl halides to their equilibrium constants when they change into each other; (2) to connect this with the constants expressing the formation of the complex anions by the union of each alkyl halide with the urazole anion, and the velocity of transformation of the complex anions; and (3) to harmonize this with the amount of free energy, and equilibrium constants, of the corresponding isomeric urazole esters which rearrange into each other. In general, whenever an alkyl halide (*n*-propyl bromide, *n*-butyl bromide, isobutyl bromide) changes reversibly nearly completely into another one (isopropyl bromide, secondary butyl bromide, tertiary butyl bromide, respectively), as Aronstein and Gustavson have found, then the former alkyl halide reacts far more rapidly with the urazole salts than does the alkyl halide which is formed by such a reversible rearrangement. We shall study this particular problem by the use of thermodynamics.

$$A\bar{n}+\dot{K}+C_2H_5I \rightleftarrows A\bar{n}\cdot C_2H_5I+\dot{K} \rightleftarrows AnC_2H_5+\bar{I}+\dot{K}.$$

This complex anion theory has now been accepted by others and has been put on a firm basis by the isolation of several double compounds, such as $AgNO_3 \cdot ICH_2CN$, which seem to have all the properties demanded by the theory.

Another problem of interest to us is the study of the action of acids, or alkalies, and water on amides, hydrazides, semicarbazides, oximes, esters, etc. According to our theory the hydrolysis of these sub-

stances should be analogous to the catalysis produced by alkalies on the reaction between alkyl halides and acids. According to this theory only the cations formed by the union of the H ion with the amide, oxime, hydrazide, etc., should be *appreciably* hydrolyzed.

$$CH_3CONH_2+\dot{H}+\bar{Cl}+H_2O \rightleftarrows CH_3C\dot{O}NH_2+\bar{Cl}+H_2O$$
$$\rightleftarrows CH_3COOH+\dot{N}H_4+\bar{Cl}.$$

This has been amply verified in this laboratory by Mr. Nirdlinger, and here we have a reaction of ions. We are studying, too, the *reverse* reactions: viz., the *formation* of amides, hydrazides, etc., by the action of ammonia, amines and hydrazines on the organic acids, esters or acid chlorides, with and without catalyzers, in different solvents, etc. We shall study especially the velocity of the reaction and the equilibrium in the gaseous phase.

In the case of the oxime, in which we have a reversible change as follows:

$$(CH_3)_2CO+\dot{N}H_2OH \rightleftarrows (CH_3)_2C{=}\dot{N}HOH$$
$$\rightleftarrows (CH_3)_2C{=}NOH+\dot{H},$$

Dr. Johnson and Mr. Desha have discovered the first reversible reaction in which the so-called third law of catalysis fails to hold, in which the equilibrium point is changed by a change in the concentration of the catalyzer.

This theory of acid-catalysis allows us to predict that such cations will be found to react with alcohol, phenols, amines, hydrazines, etc. Dr. E. E. Reid has been able to show by the necessary quantitative measurements that the amides react with alcohols in the presence of acids and form quantitative yields of esters,

$$CH_3C\dot{O}NH_2+HOR \rightleftarrows CH_3C\begin{matrix}\diagup OR \\ \dot{N}H_2 \\ \diagdown OH\end{matrix}$$
$$\rightleftarrows CH_3COOR+\dot{N}H_4,$$

and that excellent yields of esters may be obtained also from anilides and hydrazides under the same conditions. This suggests that the ordinary Fischer esterification method for the isolation of amino acids in the form of their esters from casein, horn, hair, polypeptides, etc., might be effectively modified in some cases by boiling the substance in alcoholic hydrochloric acid instead of in aqueous solutions. The ester would then be formed directly instead of through the present indirect destructive methods.

$$\begin{matrix}NH_2CH_2CO|NHCH_2CO|NHCH_2CO|NH_2 \\ RO|H\quad RO|H\quad RO|H\end{matrix}$$

The chief disadvantage would be the insolubility of the proteid in the alcohol, but we find that our difficultly soluble compounds seem to work well.

Two other reactions, worked out by Dr. J. M. Johnson in our laboratory, are recalled, the simplest explanation of which is that *non-ionized salts* are transformed into other products. Acetyl chloraminobenzene and hydrochloric acid react with a velocity proportional to the concentration of the undissociated salt present, or to the *square* of the concentration of the hydrogen ions. Two other explanations of this reaction are to be considered, but as they are at present very improbable, they will be discussed in a later communication.

$$CH_3CONClC_6H_5+\dot{H}+\bar{Cl} \rightleftarrows CH_3CONHCl_2C_6H_5$$
$$\rightarrow CH_3CONHC_6H_4Cl+\dot{H}+\bar{Cl}.$$

The other reaction is one in which imido-esters of various types yield alkyl halides when heated with halogen hydrides. In this case, too, an undissociated salt seems to be decomposed.

$$ROCH_3+\dot{H}+\bar{Cl} \rightleftarrows ROCH_3\cdot HCl \rightarrow ROH+CH_3Cl.$$

This work disproves the validity of the so-called second law of catalysis, which postulated that there must be direct proportionality between the reaction velocity and

the concentration of the hydrogen ions in acid catalysis.

Professor Bredig, who, with his students, has made the chief contributions to the theoretical and experimental development of the idea that the catalyzer forms an intermediate compound, has written me that he too has recently discovered some reactions in which the non-ionized salt seems to be transformed into other substances.

We see, then, that the question whether the *anion* or *cation* (simple or complex) or the *molecular form* of a given acid, base, salt or other neutral substance, is the *chief* constituent transformed directly into the end products depends entirely upon the relative magnitudes of the various constants, and therefore varies widely in the different problems.

Occupying our attention in this connection is another phase of this work which should certainly be a great help to us in the solution of such problems. There is no method to-day for determining the concentrations of the constituents of such a reacting system as a mixture of acetamide, hydrochloric acid and water, in which

$$CH_3CONH_2+\dot{H}+\bar{Cl}+CH_3C\dot{O}NH_3+\bar{Cl}+$$
$$CH_3CONH_3Cl+H_2O \rightleftarrows CH_3COOH+\dot{N}H_4+\bar{Cl}, \text{ etc.},$$

the salt undergoing transformation is very greatly hydrolyzed. The decrease in conductivity produced by adding acetamide to hydrochloric acid does not give us sufficient *knowns* to solve for the *unknowns* in the above equation. If some method were devised for determining the concentration of the hydrogen ions *nearly instantaneously*, then these problems could be solved. A colorimetric method involving the use of indicators seems to be out of the question with such concentrated solutions, but it may, in certain cases, work well in dilute solutions. But the use of the hydrogen electrode for the determination of the changing hydrogen ion concentration at any moment would seem to be very effective in those cases in which no disturbing side reactions take place, and this problem is engaging Mr. Desha's attention. Such a method, if successful, would be of great help in the study of a large number of reactions in acid, alkaline and other solutions.

Finally, organic chemistry is to-day a ripe field for the study of energy changes in reacting systems, as has been emphasized especially by Michael. The total energy of a system can in some cases be measured by studying the rate of formation or absorption of heat during the reaction in an adiabatic system. This has been done very satisfactorily by Bredig and Epstein in the decomposition of hydrogen peroxide by potassium iodide. Especially important is the study of free energy by the use of equilibrium and electrometric methods. A large number of organic reactions can be very easily arranged to take place in concentration cells in such a manner that the electromotive force can be readily and accurately measured. In order to study such cases completely the mechanism of the reaction must be known, and the values of all the constants, especially the equilibrium constant, must be determined accurately.

We are further using electrometric methods in the study of the question whether in the presence of platinum, iridium, etc., such substances as methane, ethylene, trimethylene, etc., can be made to dissociate as follows in substitution and addition reactions:

$$CH_2{=}CH_2+2\oplus \rightarrow C\dot{H}_2{-}C\dot{H}_2+2\bar{B}r \rightarrow C_2H_4Br_2,$$
$$CH_2{=}CH_2+2\ominus \rightarrow \bar{C}H_2{-}\bar{C}H_2+2H \rightarrow C_2H_6,$$
$$CH_4+2\oplus \rightarrow C\dot{H}_3+\dot{H}+2\bar{B}r \rightarrow CH_3Br+HBr,$$
$$CH_4 \rightarrow \bar{C}H_3+\dot{H}.$$

We see, then, that organic chemistry presents a large number of interesting prob-

lems that can and must be studied quantitatively. The solution of these problems will be not only of the greatest importance to abstract science and to industry, but will take first rank in giving an insight into the fundamental processes in plant and animal life, which involve both chemical and physical phenomena in homogeneous and heterogeneous systems.

These problems will, in my opinion and that of some of my physical chemical colleagues, be solved most easily by organic chemists, and not by physical chemists. The worker who would do great service to this branch of the science must have *as the great essential* such a broad and deep knowledge of organic chemistry that he can recognize wrong interpretations of reaction mechanisms almost by *intuition:* he must not make wrong postulations regarding reactions as Ostwald did in the *theory of indicators* and in the *catalysis of amides,* and as Euler did in the *saponification of esters,* mistakes which were corrected by *organic chemists.* Then he must turn to his physical chemistry and learn *three things well*—the mass law, thermodynamics and electrochemistry; even then he must constantly advise with some well-trained *real* physical chemist, and with some mathematician, who can to some extent understand the problems.

No man can do *trustworthy* work in physico-organic chemistry with half-way preparation, but the chemist who is well equipped will render great service to the science.

S. F. ACREE
JOHNS HOPKINS UNIVERSITY,
BALTIMORE, MD.,
December 30, 1908

BACTERIOLOGY AS A NON-TECHNICAL COURSE FOR PUBLIC SCHOOLS[1]

BACTERIOLOGY, a biological science as distinct from a useful art, has hardly received any material recognition as yet in this country. The writer would advocate the academic study of certain of its phases as illustrating the general bases of life, as a mental training, and as furnishing a field for thought.

Again realizing that sociology, in ultimate analysis, is strictly dependent upon and limited by biological laws, sociological teaching which neglects the biology and physiology of the individual neglects the study of its primal units. Hence the value of, even necessity for, the elementary biological training of the prospective sociologist. But sociology is more than the study of certain units in multiple. It is the study of the interrelations of these units. Therefore, the study of the anatomy and physiology of a single example of man (or for convenience and by analogy of the frog or the plant) furnishes no actual laboratory biological study analogous to the study of the interrelations of man in the world. Bacteriology supplies this missing link, for bacteriology deals hardly at all with the individual—almost wholly with aggregations of individuals. Symbiosis, antagonism—the effect of overcrowding, the survival of the fittest—coordination of partial efforts of different cooperating species to produce a sum total result—all these phases of sociology, chosen from many more for this address at random, can be illustrated in a bacteriological course—and with materials directly under control, subject to experimental variation and, from the rapidity of bacterial development, without waste of time.

Finally, beyond these purely scholastic views lies a practical value of bacteriology as a general study, especially for women, in that it furnishes an armamentarium in dealing with certain every-day problems of household life which come into play during at least one half of the conscious waking life of man and almost the whole of the conscious waking life of the alma mater of the family—the housekeeper and actual food provider, as the wife and mother must always be in nine tenths of the population. The writer will discuss the third of these phases, *i. e.,* bacteriology for its

[1] Presented at the Baltimore meeting of the Society of American Bacteriologists, December 31, 1908.

hygienic teachings, and especially for its technique, in more detail but still without any attempt to exhaust the subject, as a suggestion to those more actively engaged in pure teaching than himself.

Bacteriology for the General Student.—Huxley somewhere has said that if it were inevitable that every human being must at some time in his life play a game of chess against an expert, the stakes being his continued existence, then the parent or state might well be indicted for criminal negligence if no knowledge of the great game were taught. A better parable concerning the teaching of bacteriology to the general citizen could not well be offered. Surely if it is worth while for children to spend years in studying music, geography, the higher mathematics, the dead languages and many other subjects not strictly necessary to existence—most of which are never used by the great mass of the public, all of which, except in their simplest forms are quickly forgotten by the average citizen—all of which must be entirely relearned by the occasional individual who intends to become an expert in them—why should not children be taught the fundamentals of a subject of daily importance to them throughout the rest of their lives? The present teaching of physiology in the public schools is really academic, for so much of it as may have any slight reflex of physiological truth is but dimly understood by either the teacher or the child, and is at best quickly confused and forgotten. Moreover, the complicated nature of the whole teaching in modern physiology is such that even the physician can use little of it in practise and draws no deductions from it without the most exhaustive tests of his deductions before daring to apply them. What then can be the value of the deductions for leading a hygienic life which the child may draw from public-school physiology, when the fact is that he generally leaves school at fourteen or sixteen, *i. e.*, before his mental grasp is well developed? The best known investigators of the relatively simple questions of dietetics swing from one extreme to the other notwithstanding long years of intimate anatomical and physiological study in the most highly equipped laboratories. Within five years, minimum feeding; maximum feeding; complete mastication; bolting the food whole; a selected diet carefully weighed, measured and calculated; and free feeding at the dictates of the appetite have all been advocated by our "highest authorities." What can the study of a mere diagram of the intestinal tract and the learning of the names of different portions of the gut do in enabling the future citizen to decide how or what to eat? Consider the case of a native South Sea Islander if the study of a railway map of the Twentieth Century Limited, printed in a foreign language, and demonstrated by a fellow savage, be the sole available source of knowledge on the subject of railway transportation on which he is to decide what produce to ship and how and when to ship it?

Bacteriology as compared with physiology is a relatively simple matter. Its fundamentals are fixed; so far as deductions of value to the ordinary citizen are concerned, any one can make them who knows the merest rudiments. Its basic elements do not require for demonstration the elaborate apparatus and animal experimentation of the physiologist—the elements can be worked out in the kitchen—as the writer worked them out years ago. Imagine the whole population having even so much real first-hand experimental knowledge of bacteriology as a medical student at the end of his first three weeks' bacterial training. At least the principles of the transfer of unseen infection on hands and utensils would be known and the general rules as to distribution of bacteria, sterilization of foods, utensils, etc. Above all, the personal defense by the individual upon which must always rest the ultimate escape from infectious diseases would be understood and its simple methods learned. In the great campaigns now waging against tuberculosis and other infectious diseases in mankind, the education of the people is the great cry. Unfortunately this education has so far consisted chiefly in teaching the etiology, pathology, distribution and economics of disease—mere formularies to the average mind, like the statistics concerning alcohol

and intemperance in another field. The "education" ought to consist in teaching the individual how and what to *do, i. e.,* the simple bacteriological knowledge and simple bacteriological technique necessary to avoid "the swallowing, by one individual, of the discharges of other individuals." This knowledge, taught as the chief end of a simple bacteriological course, could be conveyed in lessons and class-room demonstrations, not of diagrams, but of real living bacteria, without involving the opposition engendered by anatomical or physiological demonstrations on living animals which alone will ever make physiology anything but a formula to the lay (or to any other) mind. That it should be taught in the elementary schools, and especially to the girls, is made clear by the two facts that the majority of children (about 80 per cent.) leave school before the high school[2] is reached, and that upon the girls as housekeepers and mothers must the family defense depend, as well as, by precept and example, the proper training of the children in the personal defense against infection.

Community defense against infection as contrasted with family and personal defense is a matter for the public health official, and the lack of this distinction between what is personal or family and what is public health seems to be a great stumbling block over which many earnest souls have fallen more than once. Public health must always deal largely with the prevention of the infection of public utilities with human discharges—especially infected human discharges; while family and personal defense is a matter (primarily) for the mother. When it is remembered that the great mass of all the infectious diseases of the country, especially in children, are necessarily handled and nursed, not in hospitals by trained nurses but inevitably and necessarily by mothers, so that the greatest single factor in the spread of infection from the recognized case, at the present time, outside of public utilities, is again inevitably and necessarily the mother, then the training of the prospective mother in simple bacteriologic technique, at first apparently a wild dream, becomes a most practical and serious problem. Educators in bacteriology should most earnestly ask themselves if the invaluable information concerning the existence, distribution, and above all the avoidance, of bacterial infection which to them is a mere commonplace should not be widely distributed amongst the people. Are the bacteriologists of the country doing their whole duty in confining their teaching to students of the arts of medicine, public health, industry and agriculture? Is not the time ripe for a propaganda for the teaching of bacteriology to the masses? The trained bacteriologist may pass unscathed through fifty epidemics of the ordinary infectious diseases—not because of anatomical or physiological training, not even because of epidemiological knowledge—but wholly because he understands the simple elements of bacterial aseptic technique and follows them logically and consistently. Surely it is the duty of the bacteriologist to pass on the simple technique of bacteriological asepsis at least to the people at large. It is not for the physician to do it—except at the bedside and for the individual case. It is for the bacteriological teacher to enforce personal defense against infection amongst the well before the bedside is reached—to furnish the groundwork upon which the physician may build on occasion. Such courses should be given in the public schools in such grades as to reach the children between eight and sixteen years old; these courses should consist in their simplest form of demonstrations, through use of agar or gelatin plates, of the existence, and distribution of bacteria in air, water, milk, dust, feces, etc., and especially on hands: extending somewhat in scope and in individual experimental work as the grades are ascended. Microscopes would not be essential and the necessary apparatus and media could be furnished at a very low cost.

In the high schools, gradual advance in the detail of experiments should be arranged with

[2] It seems unlikely that more than 20 per cent. of the population receives high school education; or more than 1 per cent. a university education.

quantitative experiments, possibly some species work and the microscope should be introduced.

In the university academic and especially in university sociologic courses the most intricate problems of interrelations of bacteria to each other as illustrating similar interrelations in human life might be conducted. Of course all this presupposes a bacteriological training of the teachers of the public and high schools.

For the present, the education of the mothers of the present might be attained, as suggested by Dr. Norman MacL. Harris, through lectures and simple "courses" given before mothers, in connection with settlement work or in the mothers' meetings sometimes held in connection with the graded schools: women's clubs might secure teachers ready to give short courses in elementary laboratory work. Perhaps "correspondence courses" in the great journals devoted to women might, under proper supervision, stimulate many mothers to do a little elementary bacteriology at home. However done, it is the writer's belief that until such teaching is done—and done by methods involving not merely lectures or demonstrations but personal experiments by the mothers (present and prospective) themselves, the methods of personal defense against infection will never so take their proper place as to be real factors in the suppression of disease. Only when the "cleanliness" of fresh collars and cuffs and nicely brushed hair, etc., has added to it the real cleanliness of hands free from the discharges of the toilet room will personal cleanliness mean anything in relation to infection.

H. W. Hill

Minnesota State Board of Health Laboratories

TEACHING BY THE LECTURE SYSTEM

At an open meeting held a short time ago by the Case School chapter of the society of Sigma Xi, for the discussion of subjects of special interest to the members of the instructing staff at the Case School of Applied Science the lecture system was discussed. The different ideas presented seemed to suggest that a paper on this subject might be of some interest and possible benefit. I shall not discuss the efficiency of the lecture system as compared with other systems of teaching a science, since there can be little doubt but that under existing conditions this method if properly carried out is by far the best one for most sciences; but I shall treat of the various means of carrying it out and try to indicate those which seem best suited to attain the highest efficiency.

It is not the purpose of this paper to give in detail a full treatment of the different points to be considered in presenting a science course by the lecture system, but rather to collect a few facts and ideas which may, in this way, come to some who possibly have not been placed in an environment which would demonstrate the importance of the matter, and who have therefore not put as much thought on this particular question as efficient teaching would demand. A science should be presented in such a manner as will make its particular group of natural phenomena understood with the least possible expenditure of mental exertion and time on the part of the student. The presenting of a science in this manner should be the aim of the science teacher. Further, the teacher should strive so to correlate facts and suggestion that the phenomena and their explanation should be the most easily remembered.

In most of our schools, existing conditions make the lecture system by far the best for presenting a science to a class of students. This fact is more especially true in a largely experimental science, such as in chemistry or in physics. As the teaching of chemistry has been the vocation of the writer, what follows will probably apply more to the teaching of chemistry than to the teaching of any other science. The question then resolves itself into: what is the best method for conducting a lecture course so that its qualities shall be clearness, comprehensiveness, individual completeness and individual broadness? It is not alone sufficient to give a man knowledge. The subject must be presented to him in such a manner as to interest him sufficiently to make

him exert himself to learn it. Care must also be taken to show him where he can find the more detailed information. It does not suffice to teach any subject from one point of view. The failure to present a subject broadly turns out narrow men. It is broad men the world demands and these are moulded by viewing a subject from as many different standpoints as possible. For the broadest diffusion of knowledge, men of different years should be given somewhat different standpoints and shown where to find their knowledge somewhat differently. A hard-worn path should not be followed. A lecturer who writes a set of notes which he intends to follow year after year, without revision, soon finds that his intention was unsound. A lecture on almost any portion of a fast-growing science like chemistry needs more or less revision every time it is given.

Since, therefore, the straight lecture course with no reference books, text-books or mimeograph notes gives the student but one point of view, that of the lecturer, such a course is to be censured in all cases where it is possible to give it otherwise. There may be two classes of teachers who might use this method where it is not necessary: those who have had a limited experience and who are fearful that they might, for the time being, forget some of the details which the students might wish to discuss; and those who are omniscient. The first class will soon realize the fact that no man can have everything at his tongue's end, but that a teacher must be familiar in considerable detail with the subject he is teaching if he is to be at all successful.

The lecture course with mimeograph notes by the professor may be permissible in three cases: where there is no suitable text-book or reference book, where such a book is so expensive that the teacher does not feel justified in requiring the students to buy it, or where certain directions for laboratory use are, for special reasons, to be taken from the larger reference books or from the more recent literature. The dictation of such directions is perhaps allowable in a very few cases, but not for any extended course. The dictation of a complete course of laboratory directions is little less than a waste of time both for the students and for the teacher.

When we consider carefully the lecture course accompanied by a suitable text-book or reference book, by frequent recitations or conferences, and by occasional written exercises, we shall probably find the most efficient method, at least for all experimental sciences. Let us here distinguish between text-books and reference books. A true text-book should be brief and to the point, as supposedly it is to be gone over page by page. At present, however, there are a number of so-called text-books which are so much padded and cover so much ground in detail that they are entirely unsuited for this purpose. There seems to be a tendency to use such large books as text-books, with the false idea that a better course can be given, whereas in reality the student is so confused that his course becomes a chaos of facts and directions. This of course applies more especially to beginners. What can be more absurd than the placing of books like Frezenius's "Qualitative Analysis" or "Quantitative Analysis" in the hands of beginners in these subjects? The most efficient text-books for these courses comprise few and very definite directions, with explanatory notes. In an advanced course, when the student has been fairly launched in the subject, a more complete book may be used, which is not to be considered as a text-book, but more as a reference book from which suitable exercises may be selected.

The lecture itself can not do all the teaching. It should, however, lead, interest and inspire the student. The subject matter should be covered as completely as the time will allow, but not so rapidly that the average student can not grasp it readily. In order that the student should get the most out of a lecture he should be required to read carefully the corresponding pages in the accompanying reference book before the lecture takes place. The references of the pages to be covered at each lecture should be given at the previous lecture. The student should by all means study these pages in conjunction with his notes the same day the lecture is given. and more diligently the parts which were empha-

sized in the lecture. To interest the student as well as to insure greater clearness, as many typical experiments as possible should be carried out on the lecture table if the course is a complete one. If the course is only a preliminary one these experiments may be wholly or partially omitted. The manner of presenting the experiments has a large influence in communicating inspiration to the students as well as does the personality of the lecturer. To have this inspiring effect in the highest degree the lecturer must above all use good English and so choose his words that the least possible effort on the part of the student is required to comprehend the subject. The experiments must go smoothly. No muddy, half-way experiments should have a place on the lecture table. The man who has the reputation of never having an experiment fail always tries his experiments carefully before the lecture. The giving of experiments with a three-minute preparation nearly always results in few experiments and many failures. This always gives the students less respect for and less confidence in the lecturer, and the qualities which the lecturer should endeavor to have in his lecture, those of interesting and inspiring the students, are lost.

The holding of frequent recitations is an important adjunct in order to get the best results from a lecture course. Recitations hold the student to study, emphasize the important parts of the subject, give opportunity for explaining points upon which the individual student may be hazy, and give the student practise in expressing himself. These recitations should not be the reciting of a strict page by page text-book assignment, but consultations on the subject matter outlined in the lecture, whether given in the lecture or studied in the assigned reference book, or found in other available reference books. The student should always be encouraged to look up points upon which he is not clear and the books where he is likely to find the information suggested to him.

Suppose a lecturer treats in his lectures exactly the same material as is given in the reference book, or more than is given in it. The question might be asked: what is the use of a reference book under these conditions? The reference book still has the greatest value. The student has his book and can refer to it any time he feels so inclined. He can not apply to the instructor at all times as he can to his book. The student also acquires a knowledge of the subject from two standpoints: the lecturer's and the author's. In the event of recitations he may get still another standpoint. This broadening of view is still better attained by changing the assigned reference or text-book from year to year. This can readily be done in many subjects, especially those of a general nature. This changing of reference books has admirable effects. In different years it turns out men with slightly different standpoints. In the main what they learn is the same, but they do not get it in exactly the same relation; and the men graduating from a school where this custom is practised are likely to be, as a body, broader than those from a school where it is not. Another most important effect of changing the reference book from year to year is the keeping of the teacher from a rut. A lecturer, to continue successful, must keep up to the times and must do it broadly. It would seem that the using of a different reference book from year to year, as before mentioned, is also commendable as being in keeping with a broadness of presentation by the lecturer. A set of lecture notes should necessarily be revised each year, the newer facts and discoveries inserted and the old replaced as necessary. It is imperative, and the mentioning of it here may perhaps seem absurd since it can scarcely be believed that any one in the teaching profession should lose sight of its importance. This keeping apace with the times is certainly worthy of as much thought and attention as the imparting of knowledge, if not more so.

Norman A. Dubois
Case School of Applied Science,
March, 1909

KAKICHI MITSUKURI

Advices from Japan report the death, on September 16, of Dr. Kakichi Mitsukuri, dean of the College of Science in the Imperial University of Tokyo. Dr. Mitsukuri was one

of the leaders of modern Japanese thought—perhaps the most effective scholar in his relation to public affairs which Japan has ever produced. In his special line of zoology, he was the author of numerous papers; and his influence in his own field has been still more marked by the development of many young men, and by the establishment of the Seaside Laboratory at Misaki, where much excellent work has been done by Japanese, as well as by American and European naturalists.

Dr. Mitsukuri was born in Edo, Japan, on December 1, 1857. He was the second son of Shuhyo Mitsukuri, a retainer of the former feudal lord of Tsuyama. After passing through the local schools, Mitsukuri came to America in 1873, and entered the Hartford Academy, in Connecticut. The next year he attended the Troy Polytechnic School, and two years later he entered Yale, where he received the degree of Ph.D. in zoology in 1879. In 1881, he studied zoology under Professor Balfour, of the University of Cambridge. Returning to Japan, he was appointed a professor of zoology, in 1882, in the science department of the University of Tokyo. In 1883, he received the degree of Ph.D. in zoology from Johns Hopkins University. In 1893, he was appointed councilor of the Imperial University. In 1896, he was made head of a commission for the investigation of the fur seal, and in 1897, he signed in behalf of Japan a treaty whereby Japan agreed for a certain length of time to consent to any adjustment of this matter which might be made by Great Britain and the United States. In 1901, Dr. Mitsukuri was made dean of the College of Science of the Imperial University of Tokyo. In 1907, he was awarded the Order of the Sacred Treasure, in recognition of his public relations.

Dr. Mitsukuri's grandfather, Dr. Gempo Mitsukuri, is well known as the pioneer Dutch scholar of Japan. His father was also noted, as a student of Chinese classics. His uncle was one of the most noted jurists of his time. His elder brother, now deceased, was also an eminent scholar. His third brother is Baron Kikuchi, who was president of the University of Tokyo, and afterwards at the head of the public school system of Japan. His name was changed from Mitsukuri to Kikuchi, in accordance with the Japanese custom of adoption, he having been adopted into the family of one of his relatives. A younger brother is still a professor of history in the college of literature.

Of late years, Dr. Mitsukuri was engaged in the special study of the turtles. He was also largely occupied with matters of administration. He was one of the best representatives of the scholar in public life which modern Japan has produced. He had a most thorough knowledge of the English language, and of affairs in America, and his intense sympathy with American point of view caused him to be invaluable in the preservation of good feeling between these two nations which in modern times have come to border upon each other.

This is illustrated in the following extract from a personal letter written by Dr. Mitsukuri to the present writer in the year 1900.

> The history of the international relations between the United States and Japan is full of episodes which evince an unusually strong and almost romantic friendship existing between the two nations. In the first place, Japan has never forgotten that it was America who first roused her from the lethargy of centuries of secluded life. It was through the earnest representations of America that she concluded the first treaty with a foreign nation in modern times, and opened her country to the outside world. Then, all through the early struggles of Japan to obtain a standing among the civilized nations of the world, America always stood by Japan as an elder brother by a younger sister. It was always America who first recognized the rights of Japan in any of her attempts to retain autonomy within her own territory. A large percentage of foreign teachers working earnestly in schools was Americans, and many a Japanese recalls with gratitude the great efforts his American teachers made on his behalf.
>
> Then, kindness and hospitality shown thousands of youths who went over to America to obtain their education have gone deep into the heart of the nation, and, what is more, many of these students themselves are now holding important positions in the country, and they always look back with affectionate feelings to their stay in America. Again, such an event as the return of the Shimonoseki indemnity—the like of which is

seldom witnessed in international relations—has helped greatly to raise the regard in which America is held by the Japanese.

Neither is it forgotten how sympathetic America was in the late Japan-China war.

Thus, take it all in all, there is no country which is regarded by the largest mass of the Japanese in so friendly and cordial a manner as America.

It is, therefore, with a sort of incredulity that we receive the news that some sections of the American people are clamoring to have a law passed prohibiting the landing of Japanese in America. It is easily conceivable to the intelligent Japanese that there may be some undesirable elements among the lower-class Japanese, who emigrate to the Pacific coast, and if such proves to be the case, after a due investigation by proper authority, the remedy might be easily sought, it appears to us, by coming to a diplomatic understanding on the matter, and by eliminating the objectionable feature. The Japanese government, would, without doubt, be open to reason.

But to pass a law condemning the Japanese wholesale, for no other reason than that they are Japanese, would be striking a blow at Japan at her most sensitive point. The unfriendly act would be felt more keenly than almost anything conceivable. An open declaration of war will not be resented as much.

The reason is not far to seek. Japan has had a long struggle in recovering those rights of an independent state which she was forced to surrender to foreign nations at the beginning of the intercourse with them, and in obtaining a standing in the civilized world. And if, now that the goal is within the measurable distance, her old friend, who may be said in some sense to be almost responsible for having started her in this career, should turn her back on her, and say she will no longer associate with her on equal terms, the resentment must necessarily be very bitter.

The entire loss of prestige in Japan may not seem much to the Americans, but are not the signs too evident that in the coming century that part of the world known as the "Far East" is going to be the seat of some stupendous convulsions from which great nations like America could not keep themselves clear if they would? And, is it not most desirable that in this crisis those countries which have a community of interests should not have misunderstandings with one another? It is earnestly to be hoped that the American statesmen will estimate those large problems at their proper value, and not let them be overshadowed by partisan considerations.

For my part, I can not think that the American people will fail in this matter in their sense of justice and fair play toward a weaker neighbor, and such a movement as the present must, it seems to me, pass away like a nightmare. But, if ever a law should be passed directed against the Japanese as Japanese, it will be a sorrowful day personally to me.

It was my good fortune to pass several years of my younger days in two of the great universities of America, and to be made to feel at home as strangers seldom are. I would rather not say in what affection I hold America, lest I be accused of insincerity, but this much I may say, that some of the best and dearest friends I have in the world are Americans.

But the day such a law as spoken of should be enacted, I should feel that a veil had been placed between them and myself, and that I could never be the same to them and they to me. May such a thing never come to pass!

David Starr Jordan

THE WESTERN EXCURSION FOLLOWING THE WINNIPEG MEETING OF THE BRITISH ASSOCIATION

Following the meeting of the British Association at Winnipeg a party of 180 officers and guests of the association took a most delightful excursion over the Canadian Pacific and Canadian Northern railways westward from Winnipeg.

The party left Thursday night, September 2, in a train of twelve cars, composed of nine Pullman sleepers, two dining cars and one baggage car.

On Friday, September 3, a stop was made at Regina, the capital of Saskatchewan, from 11:00 A.M. to 4:00 P.M. The citizens of Regina met the party with carriages and automobiles and showed off their young town to the best advantage. Lunch was served in the new city hall, speeches followed, and then a trip was made to see the finely equipped mounted police of this region.

At 5:30 a stop was made at the young boom town of Moose Jaw, where the party was met by a brass band and led up the main street under an arch composed of the products of the region. Supper was served in the skating rink as there was no other building in the town large enough to seat the three to four hundred citizens and guests. The party then returned to the sleepers and the train pulled out during the night and started across the Great Plains. Up to 4 o'clock

on Saturday views of fertile wheat fields of Canada were seen from the car windows, when the party arrived at one of the most attractive of the young, rapidly growing, Canadian cities of the northwest—Calgary.

A most hospitable, intelligent and energetic group of citizens met the party with carriages and automobiles and, under the direction of the mayor, carried them over a vast extent of territory and showed the great possibilities of the future city of Calgary. The prices of real estate in this region will not remain stationary twenty-four hours, according to all accounts. Supper was served in the parlors of the methodist church, after which speeches were made by the mayor, President Thompson and others. The train remained at Calgary until early Sunday morning so that the trip into the Rockies could be taken in the daylight.

Stops were made at Banff and Laggan, and these two remarkably picturesque resorts in the Canadian Rockies were seen as thoroughly as the limited time would allow. Early Monday morning the train left Laggan and went down the western slope of the Rocky Mountains across the Columbia River and climbed up the Selkirks.

A stop was made at Glacier from 9:00 A.M. to 1:35 P.M. and the party indulged in all kinds of glacial activities.

Revelstoke on the Columbia was passed Monday afternoon and the party enjoyed seeing the beautiful red salmon ascending the head waters of the Frazier River.

Tuesday morning the party reached Vancouver and took the steamer about an hour later to Victoria. The four and a half hour sail from Vancouver to Victoria was one of the most enjoyable portions of the whole trip. The steamer passed the delta of the Frazier River, then across the Straits of Georgia past the islands on the west side. The route of the steamer is a very interesting one among charming islands, with remarkable developments of very youthful shorelines.

Tuesday afternoon was spent at Victoria, and Tuesday evening was the one formal reception of the whole trip in the magnificent parliament buildings of British Columbia.

Wednesday morning the party was divided into groups, some to take automobile trips, some boat trips, and some to visit local points of interest. At 2:00 P. M. the return trip was made through the islands to Vancouver where at 8:00 P. M. Sir William White gave a public lecture on the British navy.

Thursday, September 9, was spent in the rapidly growing commercial city of Vancouver; this city being the western terminus of the Canadian Pacific Railway, and having a remarkably fine harbor which has developed at a very rapid rate in the past few years. Its commerce is increasing almost daily and the great importance of this city in the future is assured. One may call it the Liverpool of western Canada.

At 5:00 P.M. on Thursday the party went back to the sleepers and started on the return trip. Only short stops were made on the return across the Selkirks and the Canadian Rockies.

On Saturday morning the party reached the most northern point of the trip, and entered perhaps the most attractive and remarkable of the new cities of western Canada; Edmonton, the capital of Alberta, on the North Saskatchewan River. Edmonton, instead of being the jumping-off place as one might suppose from a casual inspection of the railroad map, is the meeting place of three lines of transportation: the railroad, the river and the dog-trains. The citizens of this place are building it up in a very broad-minded manner, looking to an assured future when this city will have several hundred thousand inhabitants.

The party was entertained at dinner in the large upper hall of the Roman Catholic Separate School, and speeches were made here as usual, after which the party went down to the old Hudson Bay Fort, which some of the older members remembered as the only building in Edmonton when they first came to the Saskatchewan River. A boat trip on this river showed the coal mines, gold washing, the new bridge of the Grand Trunk Pacific Railway, etc.

The party left Edmonton Saturday night over the Canadian Northern and reached Winnipeg early Monday morning.

F. P. GULLIVER

FOREST PRODUCTS INVESTIGATION

PREPARATIONS have been completed for the transfer of all the government's forest products work to Madison, Wisconsin, where the U. S. Forest Service Products Laboratory will be located, and to Chicago where the headquarters of the office of wood utilization will be established.

The new Forest Products Laboratory being erected at Madison by the University of Wisconsin, which will cooperate with the government in its forest products work and which

is to cost approximately $50,000, is now in the course of construction. The laboratory will be a fire-proof building of brick trimmed in white stone and is located near the Chicago, Milwaukee & St. Paul Railroad, with exclusive tracks and other railroad facilities. The building is expected to be ready for occupancy upon January 1. In the meanwhile temporary offices will be provided.

On October 1 the Yale Timber Testing Laboratory was discontinued and the forest service equipment there shipped to Madison. The laboratory at Washington was discontinued at the same time. The Timber Testing Laboratory at Purdue, Indiana, will be operated until the middle of December when it will be discontinued and its equipment shipped to Madison.

The offices having general supervision over all the work of the branch of products will remain temporarily in Washington. W. L. Hall, assistant forester, continues in charge of Branch of Products and McGarvey Cline will be director of the Madison Laboratory. The work of the laboratory will be assigned to five offices, as follows:

Wood Preservation, which will study all problems related to the impregnation of wood with preservatives and other substances.

Wood Chemistry, which will handle all work bearing on the chemical utilization of forest products. Wood distillation, paper pulp and other fiber products, chemical analyses of creosotes, turpentines, etc., are the principal lines handled by this office.

Timber Tests, which will have charge of all tests to determine the strength and other mechanical properties of different woods.

Technology, which will study the microscopic structure of wood, methods of seasoning and drying it, and other problems of a purely technical character.

Maintenance, which will have charge of the filing and computing. It will also be responsible for the purchase of supplies and general care of the entire laboratory.

The class of work in the laboratory may be separated into three kinds, as follows:

1. The investigation of problems in experimental research.

2. Experimental work in cooperation with commercial plants to verify laboratory experiments on a commercial scale.

3. Cooperation with outside parties for the purpose of assisting them in applying principles and processes of recognized commercial value with which the service is thoroughly familiar.

The supervisory staff of the laboratory is as follows:

McGarvey Cline, Director.
H. S. Bristol, in charge of Wood Chemistry.
H. D. Tiemann, in charge of Technology.
H. F. Weiss, in charge of Wood Preservation.
Rolf Thelen, in charge Timber Tests.
W. H. Kempfer, in charge of Maintenance.

The technical force will in part be made up of the following men:

H. E. Surface, engineer in wood chemistry.
Edwin Sutermeister, wood pulp investigations.
Jason L. Merrill, chemist.
E. Bateman, chemist.
L. F. Hawley, wood distillation investigations.
Frederick Dunlap, kiln drying investigations.
C. D. Mell, microscopist.
W. D. Brush, microscopist.
C. J. Humphrey, pathologist, detailed from the Bureau of Plant Industry.
F. W. Bond, mechanical engineer.
E. W. Ford, mechanical engineer.
C. T. Barnum, mechanical engineer.
C. P. Winslow, civil engineer.

The work of the office of wood utilization at Chicago will consist of studies of the wood-using industries of various states, the study of woods in manufacture and of the methods of disposing of mill waste, the collection of statistics on the price of lumber at the mill and at the principal distributing markets of the country, and the study of specifications and grading rules. The office will also secure statistics of forest products of importance to the experimental work of the service and the study of the movements of lumber and of the conditions of the principal lumber markets.

The personnel of the office of wood utilization for the present is as follows:

H. S. Sackett, in charge.
Hu Maxwell.

Franklin H. Smith.
Roger E. Simmons.
Charles F. Hatch.

AMERICAN ASSOCIATION OF ECONOMIC ENTOMOLOGISTS

THE twenty-second annual meeting of this association will be held in Boston, Mass., on Tuesday and Wednesday, December 28 and 29, 1909, during the week of the winter meeting of the American Association for the Advancement of Science.

For the program to be made up so that it can be included as a part of the official program of the latter association and so that it can be placed in the hands of the members before the meeting it will be necessary for all titles of papers that are to be presented to be in the hands of the Secretary November 15. The length of time desired for each paper should be stated after the title, and attention is called to the action taken at the Chicago meeting whereby the secretary was directed to request members to present each paper within a fifteen minute time limit.

An arrangement has been made whereby the meeting of the Association of Horticultural Inspectors will begin on Monday, December 27, and the meeting of the Entomological Society of America will convene on Thursday and Friday, December 30 and 31, thus preventing duplication in the time of holding these meetings and giving the members an opportunity of attending all the sessions.

It is expected that an extensive exhibit will be made illustrating the New England insect fauna, together with a special display of equipment and devices for rearing and distributing beneficial species, as well as for treating noxious ones in the field.

A large number have already signified their intention of attending the meeting and the local entomologists will spare no pains to make this the most interesting and profitable one that has been held by the association.

DR. W. E. BRITTON,
President
A. F. BURGESS,
Secretary

THE ROCKEFELLER COMMISSION FOR THE ERADICATION OF HOOKWORM DISEASE

MR. JOHN D. ROCKEFELLER has given the sum of $1,000,000 to combat the hookworm disease and has selected a commission to administer the fund which consists of

Dr. William H. Welch, professor of pathology in Johns Hopkins University, president of the American Medical Association.

Dr. Simon Flexner, director of Rockefeller Institute for Medical Research.

Dr. Ch. Wardell Stiles, chief of the division of zoology, United States Public Health and Marine Hospital Service, discoverer of the prevalence of the disease in America.

Dr. Edwin A. Alderman, president of the University of Virginia.

Dr. David F. Houston, chancellor of Washington University, St. Louis, Mo.

Professor P. P. Claxton, professor of education in the University of Tennessee.

J. Y. Joyner, state superintendent of education in North Carolina and president of the National Educational Association.

Walter H. Page, editor of the *World's Work*.

Dr. H. B. Frissell, principal Hampton Institute.

Frederick T. Gates, one of Mr. Rockefeller's business managers.

Starr J. Murphy, Mr. Rockefeller's counsel in benevolent matters.

John D. Rockefeller, Jr.

Mr. Rockefeller's letter to the members of the commission is as follows:

NEW YORK, Oct. 26, 1909.

Gentlemen: For many months my representatives have been inquiring into the nature and prevalence of "hookworm disease" and considering plans for mitigating its evils. I have delayed action in this matter only until the facts as to the extent of the disease could be verified and the effectiveness of its cure and prevention demonstrated. The wide distribution and serious effects of this malady, particularly in the rural districts of our southern states, first pointed out by Dr. Charles Wardell Stiles, of the United States Public Health and Marine Hospital Service, have now been confirmed by independent observations of the distinguished investigators and physicians, as well as by educators and public men of the south.

Knowing your interest in all that pertains to the well-being of your fellowmen, and your acquaintance with this subject, I have invited you to a conference in the hope that it may lead to

the adoption of well-considered plans for a co-operative movement of the medical profession, public health officials, boards of trade, churches, schools, the press and other agencies for the cure and prevention of this disease. If you deem it wise to undertake this commission I shall be glad to be permitted to work with you to that end and you may call upon me from time to time for such sums as may be needed during the next five years for carrying on an aggressive campaign, up to a total of one million dollars ($1,000,000).

While it would be a privilege to act in any movement which offers assurance of relieving human suffering, it is a peculiar pleasure to me to feel that the principal activities of your board will be among the people of our southern states. It has been my pleasure of late to spend a portion of each year in the south and I have come to know and to respect greatly that part of our country and to enjoy the society and friendship of many of its warm-hearted people. It will, therefore, be an added gratification to me if in this way I may in some measure express my appreciation of their many kindnesses and hospitalities.

Very truly,

JOHN D. ROCKEFELLER

The reply was signed by all the members of the commission, and was as follows:

NEW YORK, Oct. 26, 1909.

MR. JOHN D. ROCKEFELLER,
New York City.

Dear Sir: Your generous offer to pay such sums as may be needed during the next five years up to a total of one million dollars to carry on a scientific and popular campaign for eradicating "hookworm disease" has our heartiest approbation and we accept your invitation to administer this trust with a keen appreciation of the opportunity that you give us to do a great public good. We have to-day taken steps for organization and incorporated for practical work and have named this board the "Rockefeller Commission for the Eradication of Hookworm Disease."

The "hookworm" parasites often so lower the vitality of those who are affected as to retard their physical and mental development, render them more susceptible to other diseases, make labor less efficient, and in the sections where the malady is most prevalent, greatly increase the death rate from consumption, pneumonia, typhoid fever and malaria. It has been shown that the lowered vitality of multitudes long attributed to malaria and climate and seriously affecting economic development, is, in fact, largely due in some districts to this parasite.

The disease is by no means confined to any one class; it takes its toll of suffering and death from the highly intelligent and well-to-do as well as from the less fortunate. It is a conservative estimate that two millions of our people are infected by this parasite. The disease is more common and more serious in children of school age than in other persons. Widespread and serious as the infection is, there is a most encouraging outlook. The disease can be easily recognized, readily and effectively treated and by simple and proper sanitary precautions successfully prevented. The undertaking proposed by you is therefore not only full of promise of great benefit, but is eminently definite and practicable. We desire, individually as well as collectively, to thank you for this opportunity to be of service to our fellowmen and we enter upon our task with a deep sense of the responsibility laid upon us.

SCIENTIFIC NOTES AND NEWS

THE National Academy of Sciences will meet at Princeton University, beginning on Tuesday, November 16.

BIRMINGHAM UNIVERSITY held on October 20 its first congregation for the conferring of honorary degrees. The degree of LL.D. was conferred on Mr. Balfour, Mr. Chamberlain, Mr. Andrew Carnegie and other distinguished guests, including the following men of science: Sir William Crookes, Sir Archibald Geikie, Sir Joseph Larmor, Sir Douglas Powell, Sir William Ramsay, Lord Rayleigh, Professor Rutherford, Professor Silvanus Thompson, Professor Tilden and Sir J. J. Thomson.

AT the meeting of the New York Section of the American Chemical Society, on November 5, the Nichols medal will be awarded to Dr. L. H. Baekeland for his papers on "The Synthesis, Constitution and Industrial Application of Bakelite" and "Soluble and Fusible Resinous Condensation Products of Formaldehyde and Phenol."

MR. SHACKLETON was presented with a gold medal of the Royal Society of Geography at Brussels and a diploma of honorary membership after lecturing before the society on October 20.

DR. JULIUS WIESNER, professor of botany at the University of Vienna, has retired from active service.

PROFESSOR J. VOSSELER, director of the Biological Station at Amani, has been appointed director of the Zoological Garden at Hamburg.

DR. G. LOPORIORE has been appointed director of the Agricultural Station at Modena.

MR. JULIAN S. HUXLEY, B.A., late scholar of Balliol College, Oxford, has been elected to the biological scholarship at Naples for 1909. Mr. Huxley, who is a grandson of the late Professor Huxley, gained the Newdigate prize for English verse in 1908.

THE Liston Victoria jubilee prize of the value of £100, which is awarded every fourth year by the Royal College of Surgeons, of Edinburgh, to the fellow or licentiate of the college who has done the greatest benefit to practical surgery during the period since the last award, has been adjudged to Mr. Robert Jones, of Liverpool.

THE following awards by the British Institution of Mining and Metallurgy have been announced: The gold medal of the institution to Professor William Gowland, who has recently vacated the chair of metallurgy at the Royal School of Mines, in recognition of his services in the advancement of metallurgical science and education. The "Consolidated Goldfields of South Africa (Limited)" gold medal to Mr. W. A. Caldecott for his work in the investigation of methods of reduction and treatment of gold ores. The "Consolidated Goldfields of South Africa (Limited)" premium of 40 guineas conjointly to Mr. C. O. Bannister and Mr. W. N. Stanley, in recognition of their work in the investigation of the thermal property of cupels, and for their joint paper on "Cupellation Experiments."

IN accordance with the recommendations of the recent departmental committee on the West African Medical Staff, the secretary of state for the British colonies has appointed the following men to be an advisory committee on medical and sanitary questions connected with the British colonies and protectorates in tropical Africa: Mr. H. J. Read, C.M.G., of the Colonial Office (chairman); Sir Patrick Manson, K.C.M.G., M.D., F.R.C.P., F.R.S., senior lecturer, London School of Tropical Medicine; Sir Rubert Boyce, M.D., F.R.S., dean of the Liverpool School of Tropical Medicine; Mr. C. Strachey, of the Colonial Office; Mr. W. T. Prout, C.M.G., M.B., late principal medical officer, Sierra Leone; Mr. Theodore Thomson, C.M.G., M.D., of the local government board; Mr. W. J. Simpson, C.M.G., M.D., F.R.C.P., professor of hygiene, King's College, London; Mr. J. K. Fowler, M.A., M.D., D.Sc., F.R.C.P., late dean of the faculty of medicine, University of London.

DR. ALLAN KINGHORN, who has recently returned from northeast Rhodesia and central Africa, where he was sent by the Liverpool School of Tropical Medicine, together with Mr. R. E. Montgomery, to investigate the sleeping sickness there, has been appointed by the secretary of state for the colonies to proceed to West Africa where he will continue his work on the sleeping sickness. Since his return from Africa Dr. Kinghorn has been engaged at the research laboratories of the school, and together with Mr. Montgomery has completed a report of the Zambesi Sleeping Sickness Expedition. Mr. Montgomery has been appointed to a post under the Colonial Office in British East Africa at Nairobi.

BY the will of the late Mr. Mitchel Valentine, after a few minor bequests have been given, the residue of the estate, valued at $2,000,000, is to be divided equally between the Presbyterian Hospital and Hahnemann Hospital of New York.

MR. ANDREW CARNEGIE has given 450 acres of land as the site for a sanitarium for tuberculosis patients. The land is at Cresson, Pa., at an elevation of 2,400 feet.

THE Mount Weather Research Observatory of the United States Weather Bureau has just completed its first sounding balloon campaign in the west. Small rubber sounding balloons were sent up simultaneously at two points, viz., Fort Omaha, Nebr., and Indianapolis,

Ind. Thirteen ascensions were made at Fort Omaha and seven at Indianapolis. Owing to an unforeseen difficulty at the last-named place the full program could not be completed. All the meteorographs sent up at Indianapolis have been recovered and thus far about one half of the Omaha instruments have been returned.

The American Academy of Medicine will hold its third mid-year meeting at Yale University on November 11 and 12, when it will conduct a conference on "The prevention of infant mortality." There will be four sessions devoted, respectively, to medical, philanthropic, institutional and educational prevention, before which papers will be presented by distinguished speakers.

Dr. Von Brunck, of the "Badische Anilin," has made a gift of 50,000 Marks to the Munich Academy on the occasion of the fortieth anniversary of his entry in the industry.

Nature states that prizes to the value of £1,500 are offered by the National Medical Academy of Mexico for work on typhus fever. Of the sum named, £1,000 will be awarded to the discoverer of the cause of typhus, or of a curative serum, and £500 to the investigators whose work is judged most useful in helping towards such discovery. The competition is international, but all essays must be written in Spanish. They can be received up to February 28, 1911.

The Herter lectures at the University and Bellevue Hospital Medical College will be given this year by Professor Otto Cohnheim, of the University of Heidelberg. The lectures begin on Monday, December 6, at 4 P.M., and continue daily throughout the week. The subject is "Enzymes and their Actions." Those interested are cordially invited to attend.

The fourth lecture in the series under the J. C. Campbell lecture fund was given on October 19, before the society of the Sigma Xi at Ohio State University, by Professor H. T. Ricketts, of the University of Chicago. The subject of the address was "The Transmission of Disease by Insects."

On the program of the meeting of the American Philosophical Society for November 5 is a paper on "The Correlation of the Gastric and Intestinal Digestive Processes and the Influence of Emotions upon them" (with X-ray illustrations of the movements of the food in the digestive organs), by Dr. W. B. Cannon, professor of physiology in Harvard University.

Professor Otto Folin, of Harvard University, has delivered a lecture before the Academy of Medicine, Cleveland, O., on "Ten Years' Progress in the Field of Metabolism."

The American ambassador presided at the opening meeting of the winter session of the London School of Tropical Medicine, on October 26, when Professor W. Osler, M.D., F.R.S., regius professor of medicine at Oxford University, made the address on "The Nation and the Tropics."

The Bradshaw lecture of the Royal College of Physicians of London, delivered on November 2 by Professor J. A. Lindsay, was on "Darwinism and Medicine." The FitzPatrick lectures by Sir T. Clifford Allbutt, on November 4 and 9, are on "Greek Medicine in Rome."

The *Journal* of the American Medical Association states that the dedication of the monument to the memory of Professor P. Tillaux took place in the amphitheater of anatomy of the hospitals of Paris on October 29. This monument was the last work of the sculptor Chaplain. It is in marble, and represents Tillaux standing, his right hand on a half-dissected cadaver, giving his lesson in anatomy. The monument stands in the garden of the amphitheater where Tillaux was director of anatomy before being appointed professor in the medical school.

UNIVERSITY AND EDUCATIONAL NEWS

Following the acceptance of the gift by Mr. W. C. Procter of $500,000 for the Graduate College of Princeton University, the trustees have chosen as its site the land added to the campus in 1905. This tract is a short distance to the southwest of the central campus. It contains 240 acres, sloping to the

southeast on contour lines similar to those of the older campus.

TEACHERS COLLEGE, Columbia University, will purchase, at a cost of about $200,000, a ten-acre tract of ground overlooking Van Cortlandt Park, near the terminus of the subway. One third of the ground, a natural plateau about forty feet above the level of the park, will be used as an athletic field for the Horace Mann School. The remainder, a finely wooded plateau, about fifty feet higher, will be used for dormitories and houses for instructors.

DR. DONALD J. COWLING was installed as president of Carlton College at Northfield, Minn., on October 18.

DR. WILLIAM ARNOLD SHANKLIN will be installed as president of Wesleyan University on November 12.

MR. STEWART J. LLOYD has been made adjunct professor of chemistry at the University of Alabama.

LEON H. PENNINGTON, A.B. (Michigan, '07), Ph.D. ('09), has been appointed instructor in botany in Northwestern University.

AT Wellesley College the following promotions have been made: Elizabeth Florette Fisher, B.S., from associate professor to professor of geology; Lincoln Ware Riddle, Ph.D., from instructor to associate professor of botany; Caroline Burling Thompson, Ph.D., from instructor to associate professor of zoology; Alice Robertson, Ph.D., from instructor to associate professor of zoology. With the reorganization of the department of physical education Amy Morris Homans, M.A., formerly director of the Boston Normal School of Gymnastics, becomes head of the department of hygiene and physical education. Miss Homans is joined in this work by Dr. Frederick Pratt, instructor in biology and hygiene, and Dr. Louis Collin, instructor in applied anatomy.

DISCUSSION AND CORRESPONDENCE

A REPLY TO DR. PERCIVAL LOWELL

TO THE EDITOR OF SCIENCE: In your issue of September 10, Dr. Percival Lowell alleges that I have made four mistakes in my "Introduction to Astronomy," and from these alleged mistakes as premises he draws the unique conclusion that the planetesimal hypothesis "will not work." Quite apart from the validity of the allegations, it is, to me, a novel idea in logic that errors made in trying to support a proposition become thereby "disproof of it." One might infer by this sort of reasoning that the errors of the class-room have long since destroyed all the principles of mathematics. The logic of the present case is all the more remarkable in that two of the four alleged mistakes do not occur in my discussion of the planetesimal hypothesis at all, while the two that do relate to it are really one, and it is not shown that even this one has any *critical* relations to the hypothesis.

The first point raised by Dr. Lowell is in reference to the greatest and least velocities which meteors moving in parabolic orbits can have relatively to the earth, and in this discussion, which appears eighty-three pages before I have mentioned the planetesimal hypothesis, I have made an error for which I offer no excuse. In fact, it was quite inexcusable because I had fully treated, four years earlier, in my "Celestial Mechanics" (chapter VII.), the question of the motion of an infinitesimal body relatively to that of two finite bodies describing circles, and the velocity of impact of meteors is only a special case under it. If Dr. Lowell had been as generous in citing this earlier and fuller treatment as in quoting my brief remarks in the "Introduction to Astronomy," he could have omitted a considerable part of his own paper in the *Astronomical Journal,* whose method does not differ in any essential way from my exposition of the question. In fact, it would have been necessary only to have determined the constant of integration of my equation (7), page 186. But I made a mistake, and this seems to fix a new principle in logic with a quantitative function: a mistake in expounding one proposition, if made within 83 pages of the discussion of another proposition, throws discredit on the latter.

If it were not for the new logic, Dr. Lowell's second indictment would have nothing to do

with the planetesimal hypothesis, for the alleged error occurs in a discussion of the Laplacian theory in connection with the ninth satellite of Saturn. In this, I have used only the universally accepted principle of dynamics that the moment of momentum of any mass about an axis can be changed only by a couple about the same axis. I can not accept the interpretation Dr. Lowell puts on my words, nor admit the correctness of his contention.

The statements which contain the third and fourth alleged errors do, indeed, appear in my discussion of the planetesimal hypothesis. They are quoted by Dr. Lowell, one as being "on page 480," and the other as being "from pages 478 to 481." They are, however, not only a part of the same discussion, but are in a single short paragraph on the same page (480). The third alleged error is in a formula occurring at the end of the fourth alleged erroneous statement, and gives the precise condition under which the conclusion reached is true. I suppose it is a part of the new logic to divide what is indivisible by the old logic, to invert the order, to give reference to the specific page of one, and to state simply that the other lies between certain pages; or, the last may be for rhetorical effect, as it avoids the repetition of a page-number, which might become monotonous if given more than once.

Not being as yet very familiar with the new logic, I will, with Dr. Lowell's permission, treat the statements in the order in which they occur in my book. The point in question is the effect of the collision of meteoric masses upon the dimensions of satellite orbits, particularly in the earlier stages of their development. By carefully omitting, in his last quotation, the sentences in which I have given the conditions under which my conclusions are true, he has made it appear that I have made categorical statements of universal application, and he has then found examples *outside of the conditions clearly specified* where my conclusions are not true. He then asserts that this is a "disproof" of the planetesimal hypothesis.

The associated alleged error is in the formula expressing the final conditions under which my conclusions are true. Dr. Lowell's friends will regret to learn that he has been overhasty in criticizing it, considering the weighty conclusion he has hung upon his criticism. In the first place he has not quoted it quite correctly, and in the second place he starts from an erroneous equation himself. Since the linear units are not specified, the elementary principle of homogeneity of units should have shown him that the right member of his first equation is incorrect. Its left member is also inexact, due apparently to an erroneous use of the integrals of the two-body problem. If we let μ represent the mass of the satellite, his first equation should have been the inequality

$$\sqrt{\frac{1+M+\mu}{R}}-\sqrt{\frac{M+\mu}{r}}>\sqrt{1+m}\sqrt{\frac{1-e}{R-r}}.$$

Developing and omitting the negligible terms of higher order, we get precisely the formula given in my book. Consequently I stand by the conclusions reached in my book on this subject when the conditions are satisfied under which I have clearly stated they are true.

Now of the planetesimal hypothesis itself, which is much more important in the present connection, Dr. Lowell appears really to have a very excellent opinion, barring its tag and signs of parentage. In his "Mars as the Abode of Life" (1908) he says, pp. 3 and 4:

> So far as thought may peer into the past, the epic of our solar system began with a great catastrophe. Two suns met. . . . It is not to be supposed that the two rovers actually struck, the chances being against so head-on an encounter; but the effect was as disastrous. Tides raised in each by the approach tore both to fragments, the ruptured visitant passing on and leaving a dismembered body behind in lieu of what had been the other. . . . Thus, what had been a sun was left alone, with its wreckage strewn about it. Masses large and small made up its outlying fragments, scattered through space in its vicinity, while a shattered nucleus did it for core.

On page 6 he says:

> Thus they [the meteorites] proclaim themselves clearly fragments of some greater body. To the

sometime dismemberment of this orb, from which disintegration our sun and planets were formed, the little solitary bits of rock thus mutely witness.

In the *Atlantic Monthly* for August, 1909, in an article entitled "The Revelation of Evolution," on page 177, after commenting on and dismissing the Laplacian theory, he says, in introducing more recent work:

Without attempting here a picture of what probably took place, let me sketch a line or two of its reconstruction as they have taken shape at midnight to one watcher of the stars.

And on the following page we read:

From the information afforded us by meteorites we turn to another discovery of recent date, the recognition of the spiral nebulæ. . . . Now, this spectrum [that of the spiral nebulæ] is just what they should show were they flocks of meteorites—and such they undoubtedly are. They give us, therefore, the second chapter of the evolutionary history. For, from their peculiar structure, we can infer what the process was that scattered the constituents of the once compact ball whose existence the meteorites attest. They consist of a central core from which two spiral coils unfold, the starting point of the one diametrically opposite the other. Now this is what would happen had the original mass been tidally disrupted by a passing tramp. Tides in its body would be raised toward and opposite the stranger, and these would scatter its parts outward; the motion due the tramp combining with the body's spin to produce the spiral coils we see. Just as in the meteorites we have found the substances from which our solar system rose, so in these nebulæ we see an evolution actually in process which may have been our own.

To those who have read the literature of the planetesimal hypothesis as it has come forth, stage by stage, during the past decade this will sound strangely familiar; and when reading Dr. Lowell's statements about the origin of meteorites, one can not help but recall Professor Chamberlin's article in the *Astrophysical Journal* eight years ago, "On the Possible Function of Disruptive Approach in the Formation of Meteorites, Comets and Nebulæ." But perhaps Dr. Lowell does not read the *Astrophysical Journal*, which is edited and published not far from the home of that "geologist out West"[1] who "astronomically . . . is unaware that what prompted his contention, the Planetesimal Hypothesis, is mathematically unsound." The Carnegie Institution, however, is not so far "out West" that it has forfeited its claim to "be treated with respect," and in its "Year Books" of 1902 to 1907 are full expositions covering every essential element that enters into the midnight reconstruction.

From these quotations it is clear that Dr. Lowell has a real affection for the main features of the planetesimal hypothesis, and if I had not been so unfortunate as to have utterly destroyed it (according to the new logic) by the blunder in my book 83 pages before I took the hypothesis up, he might almost have reconstructed it from his own recent writings. I am wondering whether in his forthcoming book on "The Evolution of Worlds"[2] he will not give additional proof of his affection for the planetesimal theory, though perhaps under some other name, or in some nameless form, more congenial to that mysterious "watcher of the stars" whose scientific theories, like Poe's visions of the raven, "have taken shape at midnight."

F. R. MOULTON

[1] *Atlantic Monthly*, August, 1909, p. 181, footnote: "Even as this essay stood between pen and print a geologist out west, in a long letter to *Science*, has repeated, in reference to the facts here set forth, the old attacks on Darwin for daring to synthesize the facts; though the geologic facts are from Sir Archibald Geikie, our own Dana and DeLapparent, who should certainly geologically be treated with respect. Astronomically he is unaware that what prompted his contention, the Planetesimal Hypothesis, is mathematically unsound."

[2] In the advance description of this book we read: "So important scientifically is the work of Professor Percival Lowell that the announcement of a new book by him might seem to belong rather in the list of technical works than in a catalogue of general reading. Professor Lowell, however, has the rare art of conveying important and new truths in language readily intelligible to the general reader. . . . His theme is the process by which a world comes into existence, the phases through which it passes. . . ."

THE DEVELOPMENT OF THE PLANETESIMAL HYPOTHESIS

When, in 1906, the planetesimal hypothesis had reached a stage of development sufficient to warrant its introduction as a working hypothesis into text-books of geology and astronomy, it seemed to its authors worth while to draw up and place on their private files a memorandum of the several stages of cosmogonic study that had led up to the hypothesis in the form it had then taken. It was not assumed that the hypothesis had reached a final form, much less that it was in any sense then proven or that could approach proof until after a long period of trial and the closest scrutiny. On the contrary, they were then engaged in further efforts to test its working qualities and to add to its details or to modify them. It, however, seemed worth while at that stage to make note of preceding steps of progress while fresh in mind for future reference if occasion should require. Such occasion seems now to have arisen.

In the introduction to the memorandum, by way of qualifying the statements of the individual parts taken, it was noted that the mutual studies of the authors had grown up so gradually and informally, their conferences had been so frequent and so free, and their relations so intimate that it was difficult to set down with accuracy the precise parts contributed by each, or the aid rendered each to the other in working these out. The memorandum was intended to indicate merely the main individual lines of work and the leading stages of progress. A quite accurate and detailed history could be worked out, if it were worth while, from the note-books of advanced students of the University of Chicago from 1892 onward, as they were familiar with the status these studies had reached at the times their lecture notes were taken. Several of these students made computations or rendered other aid sufficient to call for notice in the papers published, among whom were A. W. Whitney, H. L. Clarke, J. P. Goode, H. F. Bain, S. Weidman, C. F. Tolman, Jr., N. M. Fenneman, C. E. Siebenthal, R. T. Chamberlin and W. H. Emmons.

In the synopsis below, the memorandum of March 12, 1906, is followed in the main, but the abbreviated phrases and references have been rounded out or recast to make them more specific and the whole brought down to date.

I. Destruction (in the Main)

Line of Approach and First Step.—To find out what effects on geological climates might be assignable to changes in the constitution of the atmosphere, Chamberlin, in the middle nineties of the recent century, attempted to test, by means of the molecular velocities involved after the method of Johnstone Stoney, the probable limits to the extent of the atmospheres in early geological stages, particularly those conditioned by the molten and gaseous states of the early earth as then commonly postulated.

These tests were found to throw doubt on the common belief in the enormous extent of hot vaporous atmospheres supposed to prevail during the gaseous and molten states of the earth. The test was then carried back to the earth-moon ring postulated by the Laplacian hypothesis where its application seemed fatal to the hypothesis. Moulton aided in his test by preparing tables of parabolic velocities for the earth at various heights above its surface and at different rates of rotation. Dr. A. W. Whitney made computations relative to molecular velocities under varying temperatures and pressures. The results were set forth in a paper read by Chamberlin at the Toronto meeting of the British Association for the Advancement of Science, August 20, 1897, and more fully in the *Journal of Geology*, October-November, 1897, pp. 653–683.

Second Step.—The conclusion that the nebulous matter of the supposed earth-moon ring could not remain in a true gaseous state, *i. e.*, with the molecules in active collisional relations to one another, under the conditions postulated for the earth-moon ring under the Laplacian hypothesis, led Chamberlin to consider the alternative conception of molecules or particles revolving in independent orbits in planetoidal fashion. Condensation from this state had previously been held, generally if not universally, to give rise to *retrograde*

rotations, whereas most of the rotations of the solar system are direct. Among the more convenient references showing the general acceptance of this view are the following: D. Kirkwood, *Am. Jour. Sci.*, XXXVIII., Nov., 1864, pp. 1–2; A. Hinricks, *Am. Jour. Sci.*, XXXVII., 1864, pp. 48–52; D. Trowbridge, *Am. Jour. Sci.*, XXXIX., 1865, pp. 25–43; A. Clerke, "History of Astronomy during the Nineteenth Century," 1893, p. 383; H. Faye, "Sur l'Origine du Monde," 1896, pp. 138–140, 164–171, 270–281; C. A. Young, "General Astronomy," 1899, pp. 568–572; Sir Robt. Ball, "The Earth's Beginning," 1902, pp. 324–347; A. Clerke, "Modern Cosmogonies," 1905, pp. 26–42. It was therefore clear that if this deduction were valid it was fatal to all hypotheses of the planetesimal type; indeed its supposed validity was probably the reason why such hypotheses had not been entertained. This apparently fatal bar was removed by Chamberlin, who pointed out that in the case of bodies moving in elliptical orbits about a common center, collision can only take place when some part of the *perihelion* section of the *outer* orbit coincides with some part of the *aphelion* section of the *inner* orbit, and that *at the point of collision* the body in the outer orbit moves faster than the body in the inner orbit, though on the average the body in the larger orbit moves slower than the one in the smaller orbit, which general fact was made the basis of the previous adverse reasoning. The way was thus opened for the construction of a tenable hypothesis on the orbital basis, including the form later called *planetesimal.* This germ of constructive work on lines previously regarded as untenable was briefly stated in the paper read before the British Association for the Advancement of Science, Toronto meeting, August 20, 1897, and published in the *Journal of Geology,* October–November, 1897, p. 669.

Third Step.—The tenability of construction on an alternative line being thus assured, the skepticism regarding the old nebular and meteoroidal hypotheses was more freely entertained and led to a search for other tests, particularly those resting on grounds other than molecular activity. The discrepancy between the slow rotation of the sun at present and the rotation it should have if it had contracted from a gaseous spheroid filling the orbit of Mercury and having the equatorial velocity necessary to shed the Mercurial ring as postulated by the Laplacian hypothesis, first came to Chamberlin's attention and led to a conference, with Moulton, late in 1899, out of which grew the more systematic inspection of the dynamics of the solar system in which the chief work was done by Moulton.

Fourth Step.—By restoring theoretically, in conformity with the laws of gases, the nebulous stages of the Laplacian hypothesis, comparisons of the several moments of momenta of the spheroid at these stages with the moments of momenta of the equivalent parts of the existing system were made by Moulton with results that seemed fatal to the Laplacian hypothesis and to all other hypotheses which had a similar dynamic basis.[1] Several other tests of a dynamical character equally adverse to the Laplacian hypothesis were also set forth in this paper.

Although the restorations of the solar spheroids at the various nebulous stages were made on the basis of the known laws of distribution of gases, with liberal margins of safety, uncertainty as to the full trustworthiness of the extension of the laws of gases to bodies of such tenuity and at such temperatures was unavoidable. To cover doubts arising from this source, independent tests were made by Chamberlin on the basis of the ratios of the masses to the moments of momenta of the spheroids and of the separated rings, respectively, using the masses and the moments of momenta of the present derived bodies, thus avoiding the application of the laws of gases; and the results were found to be equally adverse to the Laplacian hypothesis.[2]

[1] "An Attempt to Test the Nebular Hypothesis by an Appeal to the Laws of Dynamics," by F. R. Moulton, *Astrophysical Journal*, March, 1909, pp. 103–130.

[2] "An Attempt to Test the Nebular Hypothesis by the Relations of Masses and Momenta," by T. C. Chamberlin, *Jour. Geol.*, Vol. VIII., January–February, 1909, pp. 58–73.

II. Constructive (in the main)

The preceding work was chiefly destructive, but there were three notable exceptions: (1) The opening of the way to construction on planetoidal lines; (2) the determination of rather rigorous criteria that must be met in forming a tenable hypothesis, viz., *the conditions must be such as to give low mass, high moment of momentum and irregular distribution of matter to the outer part of the system, and high mass, low moment of momentum and sphericity to the central part;* and (3) the recognition that spiral nebulæ offered the greatest probability of meeting these criteria and of having at the same time a planetoidal organization.[3] A summation of the leading points made in the destructive work, together with a statement of the constructive criteria above named and of the grounds for giving precedence to spiral nebulæ in the search for an origin of the solar system, was published in Science, August 10, 1900, by Chamberlin and Moulton jointly.

Fifth Step.—Considerable futile work was done, largely by Chamberlin, in trying out the possibilities of collision between nebulous bodies as a mode of origin of spiral nebulæ, but no escape was found from the high probability, amounting almost to certainty, that the resulting orbits would be too eccentric to fit the case of the solar system in any instance that was likely to occur.

Sixth Step.—The effects of the differential attractions exerted by bodies on one another when they make close approaches were then studied by Chamberlin in the lines marked out by Roche, Maxwell and others, and found to be a promising field for hypothesis respecting the origin of meteorites, comets and nebulæ. This study included not merely the direct tidal effect on a passive body, following Roche, but also the projective effect developed in a body of enormous elasticity already under high pressure and affected by violent local explosions which were subject to intensification by the changes of gravity brought to bear on them by a passing body. It was shown that the contingency of close approach was much greater than that of collision, and that the results, (1) in the case of the disrupting of solid bodies, afforded a felicitous basis for explaining the erratic orbits of comets, the clustered fragments of the comet heads, and the angularity of the meteorites into which they are supposed to be finally dispersed; while (2) the explosive projections from suns under the influence of the passing body gave a reason for the two-armed feature of most spiral nebulæ—a neglected feature to which attention was specially called—for the spiral form, for the knots and haze, and at the same time offered a basis for inferring their dynamical state. These radical hypotheses were set forth in a paper entitled "On a Possible Function of Disruptive Approach in the Formation of Meteorites, Comets and Nebulæ," by T. C. Chamberlin, *Astrophys. Jour.*, Vol. XIV., July, 1901, pp. 17–40; also *Jour. Geol.*, Vol. IV., 1901, pp. 369–393.

Seventh Step.—With these conceptions of the origins and dynamical states of meteorites and spiral nebulæ as the bases of alternative hypotheses, a more critical study was made of the probabilities of origin of the solar system from *swarms* of meteorites of *heterogeneous* and *quasi-gaseous* organization, and, more radically, of the probabilities of the *origin of such swarms* either by concentration from a state of greater diffusion or by the dispersion of some previous body. Conditions favorable to the evolution of the solar system were not found, except when the meteoric organization took the planetesimal form.[4] Specifically, the conclusion reached was that the heterogeneous meteoritic state is "inherently moribund, passing into the gaseous state on the one hand, or into the planetesimal on the other, or, in the absence of assemblage, losing its constituents to existing suns and planets by capture one by one."[5]

[3] Chamberlin in *Journal of Geology*, VIII., January–February, 1900, pp. 72–73; Moulton, *Astrophysical Journal*, XI., March, 1900, p. 130.

[4] Chamberlin in Year Book No. 3, Carnegie Institution of Washington, 1904, pp. 195–208.

[5] *Ibid.*, p. 208.

Eighth Step.—Concurrently with these constructive attempts of Chamberlin with futile results except as based on planetoidal lines, Moulton attempted a critical review of all recorded cosmogonic hypotheses, but unforeseen conditions caused the temporary suspension of work and prevented a final treatment and publication of the assembled material.[6]

Ninth Step.—With (1) an open door for constructive work with nebulæ of planetoidal dynamics made available in 1897, with (2) the controlling criteria defined, and with (3) the limitations of tenable hypotheses narrowed by the futile work, the planetesimal hypothesis was gradually given shape and working form chiefly by Chamberlin in the absence of Moulton, as set forth in Year Book No. 3, Carnegie Institution, 1904, pp. 208–233; but this shaping of the hypothesis passed under the criticism of Moulton before publication. The spirit and purpose of this constructive work is thus stated, pp. 232–233:

> It has thus been my endeavor to develop the hypothesis into sufficient detail (1) to furnish a large number of points of contact with known phenomena and with recognized mechanical principles to facilitate testing its verity by those relations, if not now, at least in the early progress of investigation; (2) to furnish a basis for deducing the hypothetical stages of the earth that preceded its known history, and for drawing thence inferences as to the conditions of the interior which the earth inherited from the mode of its birth; and (3) to stimulate inquiry into the elements involved. In short, I have endeavored to give the hypothesis a working form under the conviction that so long as the complicated elements involved remain so imperfectly determined as at present its working value is its chief value.

Preliminary to this publication the essential features of the hypothesis had been discussed before several scientific societies and subjected to criticism. The hypothesis was also set forth by Moulton in a paper "On the Evolution of the Solar System," *Astrophys. Jour.*, October, 1905, pp. 165–181.

Later Steps.—The hypothesis was somewhat further elaborated and supplied with illustrations for text-book use by Chamberlin for the chapter on the Origin of the Earth in Chamberlin and Salisbury's "Geology," Vol. II., Chap. I., pp. 28–81, 1905, and by Moulton for his "Introduction to Astronomy," 1906, pp. 463–487.

Subsequent work in further testing, developing and applying the hypothesis has been in progress as set forth in Year Book No. 4, Carnegie Institution, 1905, pp. 171–173 (Chamberlin), and 186–190 (Moulton); Year Book No. 5, Carnegie Institution, 1906, pp. 165–172, and in later Year Books. More specifically and concretely, the continuation of investigation on lines growing out of the planetesimal hypothesis is shown by Publication No. 107, of the Carnegie Institution, entitled "The Tidal and Other Problems."

T. C. CHAMBERLIN
F. R. MOULTON

CHICAGO,
October 14, 1909

[6] Moulton in Year Book No. 3, Carnegie Institution, 1904, pp. 255–256.

AN ASSOCIATION OF AMERICAN CHEMICAL RESEARCH LABORATORIES

TO THE EDITOR OF SCIENCE: In connection with the second decennial celebration of Clark University, a special meeting was held on September 16 last "for the purpose of forming an Association of Chemical Research Laboratories, to systematically exchange chemicals urgently needed in research work." Many of your readers will doubtless be interested to know the outcome of that meeting.

Chemical research, especially organic research, in this country is greatly handicapped by the length of time it takes to import chemicals from Germany, when a need for them arises unexpectedly in course of an investigation. To quote a single opinion expressed at our September meeting, Professor Arthur Michael declared that his output of work, during the past twenty-five or thirty years, has been reduced fifty per cent. by this handicap.

Now, a chemical urgently needed in one laboratory is very often lying unused in some other laboratory. It would be gladly placed at the disposal of the investigator who hap-

pens to need it, if a definite system of communication and exchange were established between the laboratories.

Such a system will now be put in operation by the newly formed Association of American Chemical Research Laboratories, of which I have been elected secretary for the academic year 1909–10. A number of the more important chemical research laboratories, including those of Harvard University, the University of Chicago, the University of Illinois, Columbia University, the Massachusetts Institute of Technology, Brown University, the University of Toronto, and others, have already joined the association, and other laboratories are welcome to join at any time. Each laboratory desiring to join the association should send to the undersigned a copy of Kahlbaum's or some similar catalogue, with marks on the margins showing what chemicals, and roughly what quantities of them, are contained in its stock. If a preparation is needed by any member of the association, inquiry is sent to the secretary, who will return information as to where and in what quantities the chemical is to be found. Every member of the association is pledged to loan to any other member any chemical which he does not immediately need himself. On the other hand, a member borrowing a preparation is pledged to order it from abroad and to return it without undue delay to the laboratory from which it has been borrowed.

The warm approval with which the plan met when presented at the Clark University conference would seem to justify all hope for its complete success.

M. A. Rosanoff

Clark University,
Worcester, Mass.,
October 12, 1909

FAMILY RECORDS

To the Editor of Science: Last spring I asked, through Science, for volunteers from among American men of science to furnish records of certain characteristics of their families for three or more generations. The response was unexpectedly large and a valuable lot of data was acquired that is now being worked up. Much more data could, however, be used to advantage and so I make this second call for volunteers. Two sets of blanks will be furnished to each person desiring them, of which one may be retained for personal use. Information is asked concerning some 35 characteristics of each individual recorded so that the task of filling the blanks is not inconsiderable. It appears that in many families the data asked for can be obtained by taking a little trouble and the indications, so far, are that the trouble is well worth while.

It would be a great help if those who have quite or nearly filled out the "Family Records" that they received last spring should return them to me as soon as convenient.

C. B. Davenport

Cold Spring Harbor, N. Y.

SCIENTIFIC BOOKS

The Plankton of the Illinois River, 1894–1899. Part II. *Constituent Organisms and their Seasonal Distribution.* By C. A. Kofoid. Bull. Ill. State Lab. of Nat. Hist., Vol. VIII., Article I., 361 pp., 5 pl., May, 1908.

This is the second volume based on the plankton investigations made by Professor Kofoid on the Illinois River. It gives the results of quantitative, numerical and qualitative studies made on plankton material which was collected in the channel waters, chiefly at weekly intervals, during the years 1894 to 1899.

As a result of the commingling of organisms from various and diverse sources, the plankton of this river has a markedly composite character, no fewer than 528 forms being represented. Notwithstanding this large number of forms, it still does not show so great a diversity of organisms as marine plankton. Fresh-water plankton is characterized by the almost universal absence of larval forms, the exceptions being the glochidia of the Unionidæ and the larvæ of dipterous insects; by the smaller number of invertebrate groups represented; and by the smaller size of the component organisms. In spite of the smaller number of forms in this

river plankton, however, its quantity was larger than that which has been obtained from the upper strata of the Atlantic Ocean.

The phytoplankton consisted chiefly of algæ and the zooplankton of Protozoa, Rotifera and Entomostraca. Relatively the plants outnumbered the animals nearly five to one. For each of the Cladocera, there were 7 Copepoda, 95 Rotifera, 18,000 Protozoa and 86,000 plants. This large number of plants would furnish an abundant supply of food for the zooplankton.

By far the greater portion of the paper consists of a detailed discussion of the statistical data pertaining to the various constituent organisms, but space will permit the consideration of only the most important of the larger groups. Bacteriaceæ were found throughout the year, but they reached their maximum development in the winter from December to January. They sometimes became a serious menace to the fishing industry as they collected on the fyke nets in such masses that their weight and resistance to the current would break the nets.

The Myxophyceæ also contributed to the phytoplankton throughout the year. Quantitatively this group was not so important as some of the others, owing to the small size of its most numerous member, *Microcystis. Rivularia, Glœotrichia* and *Aphanizomenon,* which occur so generally in lakes, were not found in this fluviatile plankton. The Chlorophyceæ were well represented both in species and individuals and showed evidences of their adaptation to the whole range of temperature changes. The group as a whole exhibited maximum periods at approximately monthly intervals. Bacillariaceæ were found in every collection and their seasonal distribution was substantially repeated from year to year. The chief maxima were found in April-May and in November-December, with smaller maxima and minima intervening.

All the collections contained large numbers of Protozoa. The Mastigophora, which consisted chiefly of chlorophyll-bearing Protozoa, were found at all seasons of the year, but four fifths of them came between the first of April and the last of September, or during the season of growth for land flora.

While Rotifera were obtained at all seasons the number was uniformly low in winter. They showed great fluctuations at other seasons. There was an apparent tendency, however, for a vernal and an autumnal maximum. With three exceptions no maxima of any considerable amplitude were found when the temperature of the channel water was below 15.5° C.

The Entomostraca as a group were represented in all collections. The minimum number was found in mid-winter and the maximum for the year in April and May. During the remainder of the year, there was usually a series of recurrent maxima and minima which generally coincided with or approximated such periods in the other plankton organisms and often showed correlations in amplitude.

Cladocera were noted every month of the year, but they did not appear in ten of the collections. The minimum numbers were found during the period of minimum temperatures and the total varied more or less with the hydrographic changes. The Copepoda were, on an average, about five times as numerous as the Cladocera. They were perennial and had a major maximum in April-May with an occasional autumnal maximum of equal or greater proportions. By far the greater proportion of the Copepoda were young, 78 per cent. being nauplii of *Cyclops* and *Diaptomus,* and 13 per cent. immature *Cyclops.*

In the concluding chapter, Professor Kofoid states that one of the most obvious conclusions of his detailed study is that the plankton production was fundamentally rhythmic or periodic in character, viewed either in its constituent elements or as a whole. The exceptions to this rhythm were usually the adventitious forms. The cause of the periodicity was not clearly revealed. It was not correlated with the physical and chemical conditions of the water. The duration interval of the rhythms averaged approximately that of the lunar month, but showed considerable variations, as might naturally be expected, owing to the very large number of environmental factors involved.

Since the maxima of Rotifera and Entomostraca were coincident with or followed closely those of the chlorophyll-bearing organisms upon which they fed, the author reaches the conclusion that the factors which controlled the periodic growths in the food organisms would account for the rhythmic phenomena in the total plankton. If some observations made by Knauthe which seem to indicate that moonlight increases the photosynthetic activities of chlorophyllaceous organisms, and consequently their growth, be accepted, then Professor Kofoid thinks that this recurrent factor of the environment may account for the rhythmic growth of these organisms which results in the production of maxima each month at or near the time of full moon. It may be said, however, that the effect of moonlight on photosynthesis must receive further confirmation before it can be regarded as a factor of appreciable importance in the production of these phenomena.

These studies show that the Illinois River possesses an abundance of plankton material which will serve as food for the higher organisms, and this abundant supply of food material doubtless bears a very important relation to the large production of food fishes for which this stream is noted. It is also interesting to know that plankton work was recently resumed on this river after an interruption of ten years.[1]

C. Juday

Madison, Wis.

The Theory of Valency. By J. Newton Friend. London and New York, Longmans, Green & Co. Pp. xiv + 180; crown 8vo, cloth. Price, $1.60.

This little volume is the latest addition to the series of "Text-books of Physical Chemistry" edited by Sir William Ramsay. The first thirteen pages are of the nature of an historical introduction. This is followed by thirty-eight pages devoted to the theory of valency, valency and the periodic law, the valency of carbon, and Thiele's theory. Ten further chapters covering sixty-nine pages are then devoted to a somewhat detailed discussion of the valency of the elements contained in each of the groups of the periodic system. Finally, forty-three pages are taken up in the consideration of Werner's theory, electrochemical theories, and the physical cause and nature of valency. The chapters on the valency of the elements of the various groups of the periodic system are rather tedious reading, and frequently lack completeness and adequacy. Many of the statements and explanations they contain would certainly be challenged by chemists. But it must be remembered that the subject of valency has ever been warmly debated, and it is hardly to be expected that a brief résumé of it would meet general approbation. In the study of the compounds of carbon and also in the investigation and correlation of many other compounds, particularly the simpler ones, the theory of valency has been of inestimable value, and the book does give the reader this impression correctly. On the other hand, in the discussion of variable valency, and the matter of partial valencies, the author has not always been clear. The reader is here left with the idea that these portions of the subject are rather more hazy and indefinite than they actually are. The introduction and the discussion of the various theories of valency form by far the best portions of the book. It is somewhat peculiar that the theory of valency should have been chosen as the subject of a volume of a series of books on physical chemistry, for valency has always been considered as belonging to chemistry proper. As Dr. Friend's book is the only attempt of an exhaustive, systematic treatment of the subject of valency, it will no doubt be useful to mature readers who can read it critically. The various references given, though they are far from complete, will nevertheless serve very well to introduce students to the literature of the subject.

The print, paper and binding of the book are excellent, these features being similar to those of the other volumes of the series.

Louis Kahlenberg

[1] Science, Vol. XXX., p. 55.

THE MECHANICS OF BIOLOGY

WE Americans are often pointed out by the English and German and French as a people who do things in a hurry. Sometimes we are admired for it: other times otherwise. Well, we must not be too puffed up in the one case, nor, in the other, feel too solitary in our guilt. Two scientific books of much interest that have appeared here in Paris in the current fortnight are the stimulus to this sage reflection.

Last year a newspaper critic began an otherwise kindly review of a recent book by an acid reference to college professors who give a new course one year and make it into a book in the next. It is true that the particular book under review was the outcome of a college course, but President Jordan and I had given that course, with annual revision, for ten years, and before that it had already been given for five years by Dr. Jordan alone or with other colloboration. Fifteen years of ripening would seem to be a decent period —even for California fruit!

The authors of the two new French books I have referred to are at this very time, and for the first time, giving the lecture courses on which their books are based. This is at least literally true of Felix Le Dantec's "La Crise du Transformisme" (Alcan), and I strongly suspect it to be true of Georges Bohn's "La Naissance de l'Intelligence" (Flammarion). Le Dantec's book is composed of the November and December lectures in his still continuing Sorbonne course, while Bohn's course, just beginning, starts off much like his book. In his case, however, it may be fairer to look on the book as the precursor of the course.

In both cases the men are very competent to lecture or write on their respective subjects and both the books and the courses are fascinating contributions to present-moment biological discussion. Each man is an active exponent of a very advanced point of view in his field of special interest, Le Dantec advocating a rigorously mechanical explanation of vital phenomena in general and Bohn a consistently mechanical theory of animal behavior.

The impulse for Le Dantec's writing and the suggestion for his title lie in the swift growth to almost dominating position among species-forming explanations of De Vries's mutation theory. He attacks this theory with all the acuteness and vigor of his logically trained and argumentative mind. If he could only bring to its aid an equal strength of personal observation and experiment he would be an easily triumphant antagonist. But Le Dantec is more prolific of syllogisms than of original observations; is more at home in his lecture room or at his writing desk than in his research laboratory or in the field. And his argument is too often a refinement of logic or a development of terminology rather than a convincing enumeration of facts that carry their own irrefutable conclusion. One can fight for chemism in biology with metaphysic just as as one has long fought against it with the same old nicked and blunted weapon. And in either case the champion has a sorry tool.

Dr. Bohn's book is of very different type. It is the direct contrast in manner. It is judicial rather than lawyer-like, although the author has his convictions. He suspects all explanations too elaborate, but also those too simple. He deplores too much theory, calling for more facts. He analyzes and criticizes, weighs and estimates, and then seeks the simplest way out.

Bohn finds mechanical reflexes, vital rhythms and "differential sensibility" sufficient to account for the behavior of that great mass of the animal kingdom from the Protozoa up to the Crustaceans and insects. In these, with their special development of sense organs, especially eyes, behavior begins to take on its first phase of psychism—by this word being meant no dualistic conception of psyche and body but simply a certain degree of mentality or complicated functioning of nervous system, a behavior based on the association of sensations in addition to the tropismic responses. Finally, with the advent of brained animals, the vertebrates, comes the first intelligence, a dominating of the sensation associations over the mechanical reflexes, the tropisms.

Bohn would do away with the word instinct; and finalism is for him an explanation that comes into play only after a first appeal to other more mechanical inherent causes. "For Jennings," he says, "selection would be effected among the various movements of trial, and the final result would be a tropism. For us, the tropism, as Loeb has said, is something inherent, and it would be one of the elements on which selection would be exercised." Thus for Bohn the psychic evolution of animals would result, in some sort, from the struggle which occurs among the old survivals of the past (as tropisms and the phenomena of differential sensibility), and the new acquisitions (fruit of associative memory). This is made only painfully, and to a certain degree, by "*coups de revolutions.*"

Finally Bohn believes that we are sufficiently advanced in our study of animal behavior to be able to enunciate certain laws, not only for the tropisms and differential sensibility—"a conception which for the first time appears in a book of animal behavior"—but also for the associative phenomena. He holds that animal psychology is now no more open to the criticism that its "explanations," such as tropisms, differential sensibility, associations of sensations, etc., are simply labels and terminology and do not really explain the animal mechanism in its behavior, than the other sciences whose more familiar terminology, as "gravitation," "atoms," etc., seem, but in reality only *seem,* to be so truly explanations.

When Newton discovered the laws of universal gravitation, he had to confess that he had no idea as to the *cause* of this phenomenon. Is his merit less great for that? Have not his ideas, although incomplete, permitted us to build a great scientific structure that compels all our admiration, although we may be to-day quite as little advanced as to the subject of the nature of gravitation as was Newton in 1687 or Epicurus three hundred years before Christ? Now in the domain of zoological psychology, the scholars of the new school inaugurated by Jacques Loeb seek to imitate Newton in the domain of astronomy; they analyze the phenomena and establish their laws. . . . As said recently by my regretted master Giard: "The analysis which is necessary to let us master the phenomena of life furnishes us a surer base than that which tends directly to explain these phenomena."

Vernon L. Kellogg

Paris,
April

NOTES ON ENTOMOLOGY

The report[1] of M. Roubaud, who spent a year and a half in the Congo region as a member of a commission to study sleeping sickness, is before us. It goes, in much detail, into the habits and habitats of *Glossina palpalis,* the particular species of tsetse flies which is concerned in the spread of this dread disease. Agreeable to the general rule he finds that the flies are very local, rarely going more than 300 yards, but may be carried by air currents and storms to greater distances. They bite only during the daytime, and feed on a great variety of animals. There is a long account of the larva, and of its curious posterior lobes, which apparently represent the posterior spiracles. The fly deposits the full-grown larva on the ground, which crawls into the soil and there pupates. The author details a number of experiments on the effects of different temperatures on the pupæ. Species of *Bembex,* a spider (*Dolomedes*), a beetle (*Cicindela*) and ants are among the natural enemies of the *Glossina.*

A large part of the work is taken up with a study of the Trypanosomes, and five of the plates illustrate them. A long bibliography is appended to the article.

The new parts of Wytsman's "Genera Insectorum" are as follows: Fasicle 76 is by J. J. Kieffer on the small Hymenoptera of the family Bethylidæ; 30 pp., 3 pls. He considers that they are most closely related to the Scoliidæ. Fasc. 77, by the same author, treats of the small family Stephanidæ; 10 pp., 1 pl. He adopts the classification of Ender-

[1] "La Glossina palpalis; sa biologie, son rôle dans l'étiologie des Trypanosomiases," Rapport Mission d'études de la Maladie du sommeil au Congo Français (1906–1908), pp. 383–652, 8 pls., 4to, 1909, Paris, by E. Roubaud.

lein. Fascicle 78 catalogues the beetles of the Group Languriinæ; W. W. Fowler is author; 45 pp., 3 colored plates. There is very little original matter. Fasc. 79, reviewing the Ortalid flies of the subfamily Pyrgotinæ, is by F. Hendel; 33 pp., 1 colored plate. This author, as usual, gives a very satisfactory account. Fasc. 80 is on the Scelionidæ, a family of minute Hymenoptera, by C. T. Brues; 59 pp., 2 pls. Many of the species are from the United States. An appendix includes the European species recently described by Kieffer. Fasc. 81 is by Dr. K. W. von Dalla Torre on the Anoplura, or lice; 22 pp., 1 plate; a brief treatment following the arrangement of Enderlein. Fasc. 82[a] is an elaborate treatise on the popular group of beetles—the Cicindelidæ, by W. Horn; 104 pp., 4 plates and a map of geographical distribution. Fasc. 83, Carabidæ, subfamily Omophroninæ, by E. Rousseau, 5 pp., 1 plate; fascicles 84, 85, 86 are by the same author on small groups of the Carabidæ. Fasc. 87 is on the "white flies—Aleyrodidæ," by A. L. Quaintance, 11 pp., 2 pls. Fasc. 88, on the beetles of the subfamily Erotylinæ, by Paul Kunht; 139 pp., 4 colored plates, appears to be very well prepared. Fasc. 89, 91, 92 are by A. Bovie on groups of weevils; Læmosaccinæ, 6 pp., 1 pl.; Belinæ, 13 pp., 1 pl.; Gymnetrinæ, 22 pp., 2 pls., 1 of which illustrates the life history. Fasc. 90, concerning grasshoppers of the subfamily Pyrgomorphinæ, is by I. Bolivar; 58 pp., 1 plate. It is a good treatment of a group well known to the author.

THE fourth volume of the Cambridge Natural History has at last been issued. It contains the accounts of the Crustacea and the Arachnida. The Arachnida is mostly by Mr. Cecil Warburton. The treatment is not very detailed, and in some parts, as the pseudoscorpions, much out of date. The chapter on mites is entirely too short, some large families, as the Hydrachnidæ, being disposed of in four or five lines. In general it is fairly accurate, but so much interesting matter has been omitted that the reader will gain only a slight idea of the enchanting world of arachnids.

THE new entomological parts of "Die Süsswasserfauna Deutschland's" include Hefte 3 and 4 on the Coleoptera, by E. Reitter, Heft 7 on the Collembola, Neuroptera Hymenoptera and Rhynchota, by R. and H. Heymons and Th. Kuhlgatz, and Heft 12 on the Araneæ, Acarina and Tardigrada, by F. Dahl, F. Koenike and A. Brauer.

The coleopterous part deals mostly with the Dytiscidæ, Hydrophilidæ and part of the Staphylinidæ. The part on Collembola is mostly on the genus *Smynthurus;* that on Neuroptera treats the genera *Sialis, Osmylus* and *Sisyra.* The Hymenoptera include species in the Ichneumonidæ, Braconidæ and Chalcidæ. The Rhynchota by Kuhlgatz is rather more complete than the other parts, with full descriptions of each species. The part on *Corixa* will be of value to the American hemipterist. Heft 12 on the Arachnida is almost wholly occupied by the water-mites, Hydrachnidæ, and is very fully illustrated.

ANOTHER part of the Catalogue of the Diptera of the world by Dr. C. Kertész is now before us.[2] This part contains the Bombylidæ, Therevidæ and Scenopinidæ. For this latter family Dr. Kertész has adopted the name Omphralidæ. The Bombylidæ include 1,696 species in 84 genera, the Therevidæ include 270 species in 16 genera, and the Scenopinidæ have but 28 species in 4 genera. *Conophorus* is used for *Ploas,* and *Argyramœba* for *Spogostylum* of the Aldrich catalogue. Especially commendable is the brief and logical form of his references.

A MONOGRAPH of *Myrmecophila,* a curious genus of crickets, is the theme of Fritz Schimmer.[3] A considerable part is devoted to the ethology and distribution of the genus, and especially to its relations with certain ants. The morphology and anatomy occupies the bulk of the paper, while in the systematic part are the descriptions of the eleven species, five of which occur in the United States.

[2] "Catalogus Dipterorum hucusque descriptorum," Vol. V., Budapest, 1909, 200 pp.

[3] "Beitrag zu einer Monographie der Gryllodengattung Myrmecophila Latr.," *Zeitschr. wissenschf. Zool.,* Vol. 93, pp. 409–534, 3 pls., many text figs., 1909.

H. Schouteden has published another fascicle of his work on the Hemiptera of the Congo.[4] A list is given of all the species now known from that region, nearly 300 in all. The plates illustrate the new species.

Nathan Banks

SPECIAL ARTICLES

THE POSSIBLE EFFECT OF CEMENT DUST ON PLANTS

A short time ago my attention was called to an extraordinarily abundant deposit of light gray dust on all sorts of exposed surfaces out of doors in one of the valleys not far from San Francisco. This dust was declared to come from the manufactory of Portland cement owned and operated by the Cowell Lime & Cement Co., near Concord, California. Unfortunately the manufacturing plant was shut down during the time in which both of my visits fell, so I did not myself see that the dust came from those works and only from there. I have, however, no reason to doubt its source, the attorney, manager and other officials of the Cowell Company admitting that they lose much cement as dust.

The light gray dust forms, where reasonably undisturbed, a film of increasing thickness over everything out of doors. This film adheres to some surfaces much more closely than to others, according to the smoothness, hairiness, stickiness, moistness of the surface. It could not be entirely blown off any surface which I saw, but the rain which fell in the interval between my two visits washed it off some surfaces, but not by any means all. Where the dust fell on undisturbed soil it could be readily recognized because of its color and shade: it is a light gray, whereas the soil is brown. Although the roads are lighter in color than the fields, since they are partly macadamized with a light gray stone, they too are darker than the dust. The origin of the dust is, therefore, clearly not entirely from the roads or fields.

In composition the dust presents some interesting characters under the microscope. It is evidently composed of fine particles of at least three different sorts. One of these is translucent crystalline fragments, fairly numerous. Another sort, less numerous, consists of somewhat larger opaque and fairly rounded particles. By far the largest number, however, are minute granular particles which cohere in irregular masses, often of considerable size. The masses of coherent granular particles enclose and in a way bind together the particles of other sorts. The granular material readily dissolves, with effervescence, in hydrochloric acid, even dilute, but the other particles remain on the slide, under the microscope, undissolved. Acetic acid similarly affects the dust when applied to small quantities on the slide under the microscope. From this it is evident that the dust consists largely of some readily decomposed carbonate.

This dust more or less completely covers the foliage of the native and cultivated plants in a considerable area, extending, as I observed, to a distance of over six miles from the cement works. It is carried on the winds and, as is so common in this part of California, the winds prevail in very definite directions according to the season of the year. In consequence, the dust goes in one direction mainly during the summer, and leaves the remainder of the valley free. It is more abundant on the windward than the leeward side of scattered trees, of orchards, etc. It covers the upper surface of many leaves, such as oak, willow, grape, prune, plum, quince; but such glossy leaves as peach, lemon, orange do not hold it against a breeze. It adheres also to the under side of many leaves, especially if the under side is less smooth than the upper. On fruits it is also evident, especially on dark or dark-skinned sorts, and it can not be removed from them without also rubbing off the bloom; it will not simply drop off if they are dipped or washed in water.

The market value of property has naturally been influenced by this excess of dust; the salability of land within the affected district being greatly decreased, and the market for otherwise fine table grapes covered by a deposit of grit is altogether a limited one.

On the other hand, it may be questioned

[4] *Ann. Musee du Congo Belge, Zool.*, Ser. III., Sec. II., Tome I., fasc. 1, pp. 88, 2 col. plates, 1909.

how great or even what may be the injury to vegetation. Bearing in mind that leaves are the parts of the more highly developed plants in which food is made under the influence of light, and through which that exchange of gases takes place which corresponds with the more mechanical part of the process of respiration in our own bodies, we see at once that these functions of leaves may be interfered with by dust. The exchanges of gases in food-manufacture and in respiration take place mainly through the openings, known as stomata, in the epidermis of leaves. If these openings are stopped or are closely covered, obviously the passage of gases through them will be correspondingly more difficult, slower and less adequate. This will be possible, however, only if the particles of dust correspond in shape, size and position to the stomata, or are so compacted on the surface of the leaves as to cover them. Examination shows that some of the particles are small enough to clog the stomata and that they do so on the leaves of oak (*Quercus lobata*), fruit-trees and grape-vines. The coarser particles form more or less extensive crusts, thus covering over the stomata. Although naturally most of the dust settles on the upper side of the leaves, the lower side does not entirely escape the clogging or crusting over of its pores. But on neither the upper nor the lower surface is the covering of dust so thick and opaque as greatly to interfere with the passage of light to the inner tissues of the leaves.

The effect, then, of an accumulation of dust on the surfaces of leaves constitutes mainly a mechanical interference with the proper exchange of those gases concerned in respiration and in food-manufacture in plants. There is no evidence that the dust on these leaves has exercised any corrosive or otherwise poisonous influence on the tissues, and so far as my preliminary tests indicate, there is no evidence of the presence of injurious substances in the dust. The effect of the dust is mainly, if not wholly, mechanical. But, interfering with the supply of food-materials and with the proper aeration of the plant-body, it must be more or less injurious.

Furthermore, whatever the effect of the dust may be on leaves already grown and developed, it is certain to be greater on young and growing leaves. The cement plant in question has been in operation only a few months, since the season's foliage was developed. The effect on young parts is not known. It may be anticipated, however, that the *setting* of this material on the rough or hairy surfaces of young and growing leaves would not only interfere with the exchange of gases above mentioned and absolutely necessary to the health of plants, but would offer a mechanical hindrance to growth which would lead to distortions more or less serious.

Perhaps it will be objected that what I have said is not probable, in the light of experience elsewhere. For example, Haselhoff and Lindau express the opinion that cement dust does no harm to vegetation:[1] but they speak of the rains which wash off the foliage at frequent intervals during the German summer. In this part of California, on the other hand, conditions are quite different. There is practically no rain after the leaves of deciduous plants have developed, although there may have been abundant rain before then. The leaves are not washed off frequently; usually they are not washed off at all throughout the season. In this part of California there are frequent summer fogs which give very valuable moisture to vegetation.[2] These fogs would affect ordinary dust very little, if at all, but they would tend to *set* dust containing or largely composed of cement. In dry weather cement dust will tend to blow away, especially in high wind, but it will be held more or less firmly, in spite of wind, on leaves roughened by hairs or made sticky by the wounds of aphids. Any moisture, whether from the plants themselves or from the air, which does not come with sufficient force to *wash,* will tend to fix the dust, forming a more or less permanent crust. Indeed, the rain which fell with considerable force on one occasion between my two visits removed far less dust from the surface of

[1] Haselhoff & Lindau, "Die Beschädigung der Vegetation durch Rauch," Leipzig, 1903.

[2] See a forthcoming paper by me in *The Plant World* on the botanical conditions of this region.

plants than I had expected. The leaves of grape were fairly cleaned, but the fruit was not, and the effect on the leaves of plum, prune and oak was slight: the dust was on them in quantity. This rain, furthermore, came unusually early.

The climatic conditions here being so different from those elsewhere, I feel compelled to expect the results which I have indicated, unless the operation of the cement manufactory is so modified as to check the discharge of dust in quantity.

George J. Peirce
Stanford University, Cal.,
October 1, 1909

The Algæ of the Ithaca Marshes

Investigations on the fauna and flora of the marshes of the upper Cayuga Lake Basin during the past summer were continued at the Biological Field Station of Cornell University. Various studies were in progress. The writer undertook the study of the algæ. His investigations were made in connection with the work of Dr. J. G. Needham, and were directed toward the solution of the problem of the algal food supply of herbivorous aquatic animals that are used as food by fishes. No local data being available, a preliminary study of the algæ of the marshes was undertaken. With little variety of conditions, a great number of species was not to be expected; however, nearly all the genera of the commoner fresh-water algæ were represented, and perhaps a more thorough search would supply the missing ones. Over seventy genera were found, but the species were not all carefully worked out, owing to the unsatisfactory state of their literature. Material for future work on them has been preserved and will be worked over later.

The genus *Chætophora* is especially abundant in this region, and is represented by four species, three of which, *C. elegans, C. incrassata* and *C. pisiformis,* are very common. The dominance of this genus, and the fact that it is used extensively as food by aquatic animals that have importance as food for fishes, lead us to expect that it will be of some economic value and a special study of its habitat and capabilities of increase will be made in the future. The study of the optimum conditions for the development of several species of *Spirogyra, Mougeotia* and *Chætophora* was undertaken in order to facilitate the cultivation of these algæ, should they prove of economic importance. The results from these experiments, and from observations carried on in nature, lead us to believe that such forms might with proper facilities be raised on a large scale with good results; perhaps in such quantities as Dr. Needham's previous studies of artificially reared may-flies indicate may be demanded.

A beginning was also made in the study of the periodicity of the marsh algæ. It is hoped that facilities may be provided in the future for carrying on observations continuously for several years, as this seems to be the only way in which exact information concerning the ecology of the algæ can be obtained.

An interesting Phycomycete, parasitic on *Volvox globator,* was discovered during August, and this fungus will be described after further work has been done on its life history.

H. A. Anderson

The Molecular Constitution of Solids[1]

According to the author, it is supposed and generally so stated, owing to complex movements and forces supposed to be present in solid bodies, there must be special difficulties to which it is due that the molecular thermodynamics and kinetics of solids can not be fully accounted for on the same basis as those of the gaseous bodies.

The present paper is calculated to show that the supposed difficulties are largely imaginary and that there are a notable number of solid and liquid substances, for which, according to the calculations and tables presented by the author, no essential difference exists regarding the mode and kinetic energy of the motion of their molecules as compared with those of gaseous bodies.

[1] Abstract of paper presented at the regular meeting of the Chicago Academy of Sciences, July 27, 1909.

In support of this, it is shown that the total heat energy at the melting point of metals in which the molecule probably consists of one or but few atoms can be found by doubling the latent heat of melting ($2l$) while the total heat energy of the liquid at the same point can be found by trebling the latent heat of melting ($3l$).

The author further shows that the amount of heat energy of a solid body thus calculated is equivalent to the total kinetic energy ($Mv^2/2$) of the molecules, calculated from the molecular velocity of the body, which velocity is determined in the same manner as is done in case of gaseous bodies in accordance with the kinetic gas theory.

As another proof of the author's assertions it is shown that the heat energy of many solid bodies calculated by multiplying the absolute temperature degrees of the melting point with an amended specific heat (Ts) is found to be practically identical with the energy quantities ($2l$) and ($Mv^2/2$) found as before stated.

Tables are submitted illustrating these conditions for mercury, silver, cadmium, tin, bismuth, potassium and platinum.

It is also shown in another table that chemical combinations such as water, saltpeter and chloral-hydrate follow the same rule if the splitting of their molecule at melting is duly considered.

To further establish the identity of principles of the kinetics of solids and gases a third table demonstrates that the interior energies of the permanent gases can be calculated with correct results on the basis of molecular velocities deducted from the latent heat of melting of solids.

As another corollary an original experiment is quoted according to which a drop of water is suspended in a liquid of equal specific gravity below its freezing-point when the globular form of the drop at once changes into an ellipsoidal form as soon as it freezes, showing that the abstraction of the latent heat of melting is accompanied by a great loss of energy in the perpendicular direction.

Some of the principal results of these investigations may, according to the author's summary, be itemized as follows:

1. The absolute zero of temperature —273° Cels. as derived from the kinetic conditions of gases may be derived from the kinetic conditions (molecular movements) of solids by identical processes of reasoning and calculation.

2. In the case of many solid elements, as well as also for many combinations of simple constitution, their total internal heat energy is chiefly kinetic energy approximately expressible by the product of their corrected specific heat with the absolute temperature (Ts_1).

3. The heat energy of such solid elements at their melting-points may likewise be expressed approximately by doubling their latent heat of melting ($2l$) and in the case of chemical combinations in which the molecule splits during the melting by $1l$. The total heat energy of the liquid body at its melting-point being three times its actual latent of melting ($3l$).

4. The molecular velocities of solids may be calculated from the same principles on which the kinetic theory of permanent gases is based.

5. The total kinetic energy of a body calculated according to these velocities is approximately the same as that derived from the latent heat of melting and also the same as that derived from the corrected specific heat multiplied with the absolute temperature.

6. The three different arithmetical expressions for the kinetic energy of a solid body at its melting-point, viz., Ts $2l$ and $Mv^2/2$, every one of which is derived from different experimental data and by different processes of reasoning give approximately identical results, thereby making all conditions of matter amenable to the same general kinetic principles, at least with respect to the substances under consideration in this treatise.

7. The molecular constitution of solid bodies is not essentially different, and in some cases apparently even less complex, than that of gaseous bodies, so that the translatory

energy of elastic spheres alone may suffice to account for their kinetic conditions in many respects.

J. E. Siebel

Chicago, Ill.

SOCIETIES AND ACADEMIES

THE ANTHROPOLOGICAL SOCIETY OF WASHINGTON

The 434th regular meeting of the society was held October 12, 1909, in the West Hall of the University Building, George Washington University, with President Fewkes in the chair.

The paper of the evening was by Dr. Ales Hrdlicka, whose subject was "The Anthropology of Egypt in the Light of Recent Observations." This dealt mainly with the results of Dr. Hrdlicka's expedition to Egypt, carried out during the first part of the current year under the auspices of the Metropolitan Museum of Art and the National Museum. The expedition brought, besides other results, very important additions to the skeletal collection in the National Museum.

The speaker, after pointing out the importance to anthropology of studies on the Egyptians, and mentioning the important work that has been and is being done in Egyptian research and exploration by American men of science, particularly Breasted, Reisner, Lythgoe and Davis, proceeded to discuss the results of recent investigations on well identified skeletal remains from that country, ranging from the earliest to the latest periods, and also the results of his examinations of the living remnants of the Egyptians.

The principal facts brought out were as follows:

Contrary to the hitherto prevailing opinion, there were, in the course of time, marked changes in the physique, particularly the form of the skull, of the Egyptians, the ancient crania being, on the average, decidedly longer and narrower than those of the later periods.

The Egyptians originated, in all probability, from more than one stream of anthropologically related people, the principal elements being Libyan and Puntite or Arabian.

Negro admixture was very small up to the time of the empire. A more noticeable addition consisted of a brachycephalic strain coming probably from Asia Minor. This is traceable from the earliest times and became important during the Greek and Roman occupation.

The Egyptians were light to medium brown in color, usually with black, straight to slightly curly hair, a moderate stature and muscular development. They approached closely in all their important features the north Africans and southwestern Asiatics and with these the European dolichocephalic whites.

At the present time the ancient Egyptians may be considered as practically extinct or rather obliterated. The actual population of the country is an amalgamation of the original inhabitants with the Arabs, Negroes and many elements from the northwest, north and northeast of the Nile Valley.

In certain localities traces of the old Egyptians still can be recognized. This seems to be principally the case in the great oasis. A series of 155 natives of this oasis were closely studied, also photographed. The data are being prepared for publication.

The paper was discussed by Professor Holmes, Dr. Casanowicz, Dr. Folkmar, Dr. Hough, Dr. Fewkes and others.

John R. Swanton,
Secretary

THE AMERICAN CHEMICAL SOCIETY. NEW YORK SECTION

The first regular meeting of the session of 1909–10 was held at the Chemists Club on October 8.

Dr. Morris Loeb, in taking the chair, made a short address. He spoke of the plans of the section for the approaching session and especially commented upon the desirability of a chemical museum, expressing the hope that it might be housed together with the society library in the new quarters of the Chemists' Club.

Dr. A. P. Hallock gave a report of the general meeting of the society at Detroit, calling attention to the hospitality and very enjoyable entertainment offered by the Detroit Section.

Professor M. A. Rosanoff, of Clark University, read a paper by C. W. Easley and himself "On the Partial Vapor Pressures of Binary Mixtures." This paper is published in full in the current number of the *Journal of the American Chemical Society*.

Professor H. C. Sherman presented "A Source of Error in the Examination of Foods for Salicylic Acid."

The members present were invited to attend a special meeting of the Chemists' Club held after the adjournment of the meeting of the section. At this meeting the plans of the building committee for a larger and more adequate club house were adopted, insuring a home for the chemists of New York unexcelled in this country or probably abroad.

C. M. Joyce,
Secretary